EARTH STRUCTURE

THE EVOLUTION OF EARTH STRUCTURE

WITH A

THEORY OF GEOMORPHIC CHANGES

BY

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INTRODUCTION

The problems of Earth-Structure which are dealt with in this work are the outcome of many years' study. I have attempted to arrange the matter in such a way that, while making a detailed investigation of each problem, the inception and growth of the essential underlying idea should become apparent. My Presidential Addresses here reprinted on the 'Denudation of the Two Americas' and the 'Atlantic as a Geological Basin' contain the germs of some of the leading principles advocated, and which I am fain to believe are natural developments of the 'True Principles of Geology' as expounded by HUTTON, LYELL, and indirectly by CHARLES DARWIN. The application of dynamical principles to the explanation of the facts of geology is the most modern phase of geological investigation, and it is to this branch that my principal studies have been directed. Further introductory remarks would be superfluous, as the problems carry their own explanation.

To the Geological Society of London my thanks are due for the encouragement afforded by the

award of the Murchison Medal in 1896, and to the Liverpool Geological Society for permission to use the various papers by me that are published in the pages of its Proceedings, and for the sympathy and help the Society has never failed to extend to me in my scientific labours.

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In the conduct of scientific investigations such as are recorded in this volume the author is thrown into communication with numerous fellow-workers, and the interchange of ideas which takes place is of the utmost value. It is my wish here to record my grateful thanks to all who have in any way assisted me, and I hope it will not be considered invidious if I single out the following names for special acknowledgment. Among my actual co-workers I must mention Mr. Philip Holland, F.I.C., my son M. Treleaven Reade, and Mr. Maynard Hutchings, F.G.S.

By intercommunication of ideas, both in the field and in the study, I have probably learned most from my friend Dr. Charles Callaway, F.G.S.; while to Dr. Henry Woodward, F.R.S., my warmest acknowledgments are due for the universal kindness he has displayed during my scientific career and the impartial way in which he has thrown open the pages of the 'Geological Magazine' to my communications and freely conceded their re-use in this work.

Discussion with the Rev. F. F. Grensted, M.A.,

on the physical principles underlying my theories has proved a great help.

To these names I may add those of Mr. Charles Moore, F.I.C., and the late Mr. Norman Tate, F.I.C. For reading proofs and literary help I am indebted to my son Aleyn Lyell Reade, and to Messrs. Hartley Brothers, of Waterloo, for the skilful way in which they have surmounted the difficulties of photographing the experimental models.

T. Mellard Reade.

Park Corner, Blundellsands:
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BOOK I

GEOMORPHIC CHANGES

CHAPTER I

AN INQUIRY INTO THE CAUSE OF REGIONAL OSCILLATIONS OF THE LEVEL OF THE EARTH'S SURFACE, WITH A THEORY OF GEOMORPHIC CHANGES

PART I

GEOLOGICAL EVIDENCES OF BENDINGS AND CHANGES OF LEVEL OF THE EARTH'S CRUST

VERTICAL Movements Classified.—That there have been oscillations of the level of the land relative to the mean sea-level is a fact that attracted the attention of early philosophers and geologists.

This was roughly inferred from the discovery of marine fossils in rocks thousands of feet above the sea-level.

As the science of geology advanced these marine exuviæ, it was found, had been in one class of cases lifted into their positions by the folding of the rocks in which they were entombed and

their elevation into mountain ranges by lateral pressure; and in another class of cases by the direct elevation—differential it may have been—of whole regions to be measured by thousands of square miles.

Again, it was found that in all but the more recent formations, such as the Pleistocene, the rocks had undergone so many vicissitudes and movements since they were laid down on the shores or beds of the sea that it was difficult, if not impossible, to tell whether the last movement had been one of elevation or depression. In fact, the only safe inference that could be drawn was that the vertical movements the earth's crust had undergone, as exhibited on the dry land, were multitudinous. The fossils, such as the nummulites found in limestone of Eocene age in the Alps, Pyrenees, Caucasus, and the Himalayas, and up to 16,500 feet in Western Thibet, had attained such extreme elevation through the rocks in which they were enclosed having been involved in the mountain-building.

Raised Beaches.—Later investigations have, however, shown that numerous vertical movements have taken place in the British Isles in what are called Pleistocene times, some being pre-glacial, others glacial or post-glacial, or even recent. These are marked by raised beaches, buried river-channels, or submerged forests.[1]

[1] See 'Oscillations in the Level of the Land as shown by the Buried River Valleys and later Deposits in the Neighbourhood of Liverpool,'

The proofs of these movements are not to be questioned by any sane investigator.

Let any one who doubts visit the 40-foot beach on which the town of Irvine, in Ayrshire, is built, and examine the constitution of this considerable raised plateau or delta as shown in the banks of the Irvine Water, where it cuts through these deposits, and I venture to predict that he will return convinced.[1]

This raised beach is to be found repeated on the east coast of Ireland, and may be seen at Larne. It also is found on the northern shore of the Isle of Man covering several square miles, though the uplift is small—not more than 10 feet. There appears to have been a neutral axis to this uplift, stretching across the Irish sea south of the Isle of Man, the north of this axis being an uplift increasing in vertical amplitude to the north, the south a depression increasing in amplitude southwards. The shores of the Bristol Channel and of Cornwall and Devon on the English Channel show evidences of considerable subsidence.[2]

Again, it has been conclusively shown by Spencer, Gilbert,[3] and others, that the region of the

read before Section C, Liverpool meeting of the British Association, and published in *Geo. Mag.*, Nov. 1896, pp. 488–92.

[1] See 'Geological Position of the Irvine Whale Bed,' J. Smith, *Trans. Geo. Soc.*, Glasgow, vol. x., no. 5, pp. 29–50.

[2] See 'The Peat and Forest Bed at Westbury-on-Severn,' *Proc. Cotteswold Naturalists' Field Club*, vol. xiv., Dec. 1901, pp. 15–46.

[3] In this connection see 'Notes upon the Origin and History of the Great Lakes,' *Proc. Am. Assoc. Adv. Sci.*, vol. xxxvii. (1888); 'A Review of the History of the Great Lakes,' *Am. Geologist*,

Great Lakes of North America has undergone warping or differential vertical movement, by which the originally horizontal shores of the Lakes have become inclined, and it is considered by these geologists that the movement is still in progress. Gilbert estimates the mean rate of tilting at 0·42 foot per 100 miles per century.[1]

Former subsidence is also shown by the fact that Lake Ontario, which is now only 247 feet above the sea, is 738 feet in its greatest depth, or 491 feet below sea-level; and there are other evidences of changes of level and differential land movements.

On the continent of North America, it would appear from the observations of Dr. G. Dawson that the Rocky Mountains exhibit evidences by raised beaches at levels of from four to five thousand feet of a former submersion to that extent; and that this movement was differential he infers from the presence of Laurentian rocks, derived from the Laurentian Mountains.[2] Though hitherto no marine fossils have been discovered in these Pleistocene beaches, it is difficult to resist the conviction that the terraces have been formed by the sea.[3] Evidences of former submersion have

vol. xiv., Nov. 1894 (further references given on p. 295 of this last paper).

[1] 'Modification of the Great Lakes by Earth Movement,' *National Geo. Mag.*, vol. viii., 1897, p. 245.

[2] 'Glacial Deposits of South-Western Alberta in the Vicinity of the Rocky Mountains,' *Bul. Geo. Soc. of America*, vol. vii. p. 37 (1895).

[3] See *Physical Geography and Geology of Canada*, pp. 46 and 47 (Reprinted from the *Handbook of Canada*, *Brit. Assoc.*, 1897).

also been observed near Mount St. Elias, in Alaska, by Israel C. Russell, up to 4,000 to 5,000 feet.

The Chaix Hills, though composed of soft, easily eroded strata, stand out in sharp ridges surmounted with irregular pyramids, indicative of immature sculpture; this indication of youth is also sustained by the fossils with which many of the strata are charged, which are of living marine species.

The most striking feature of these hills is that for a thickness of from 4,000 to 5,000 feet they are composed of stratified morainal material. As the beds are tilted northwards at an angle of 10 or 15 degrees, it would seem that the elevation was of a sharply differential nature.[1]

Still later Reginald A. Daly has described the occurrence of raised post-glacial shore-lines in Newfoundland and Labrador, ranging from 575 feet at St. John's to 250 feet at Nachvak. Furthermore, Mr. Daly gives a section between these two points, showing in graphic form the differential bendings that have taken place along this uplift of eleven hundred miles of coast.

In his opinion, 'the pronounced warping of the highest shore-line is incompatible with the view

[1] 'Second Expedition to Mount St. Elias in 1891:' *Thirteenth Annual Report of the Directors of the U.S. Geo. Survey* (p. 24 of reprint).

In 'Reconnaissances in the Cape Nome and Norton Bay Regions, Alaska, in 1900,' what were considered to be elevated marine beaches were discovered at various altitudes from 200 to 1,700 feet, from which the authors (Alfred Brooks, Richardson, Collier, and Mendenhall) conclude that in comparatively recent times the western province was submerged to a depth of 1,000 feet or more. Photographs accompany the description. *U.S. Geo. Survey, Dept. of the Interior*, p. 58.

that changes in the position of the level of the sea over great stretches of the earth's surface are produced solely by independent vertical movements of the surface of the ocean.'[1]

In Greenland, undoubted sea margins with marine shells of recent species occur up to 1,000 feet, and Colonel Feilden's observations go to prove that there has been a general movement of upheaval of the land which surrounds the North Pole, as previously pointed out by Sir Henry Howorth.[2]

Colonel H. W. Feilden, in a valuable series of papers on the 'Glacial Geology of Arctic Europe and its Islands,' shows that the marine elevated terraces of Norway extend northwards, and that the southern shore of the island of Arnö 'is fringed for miles by three great parallel terraces,' which he estimates as 50, 100, and 150 feet above sea-level; while at both sides of Varanger Fiord, which separates Norway from Russian territory, a well-known series of terraces occurs.

The island of Kolguev, composed entirely of sand and clay, is in itself a striking evidence of elevation. In Novaya Zemlya, Franz Josef Land, and Spitzbergen raised beaches are frequent, and it is quite evident that a vast area to the north of Russia in Europe, including the bottom of the Arctic Sea, has been elevated in comparatively

[1] 'The Geology of the North-east Coast of Labrador,' *Bul. Museum of Comparative Zoology at Harvard College*, vol. v. no. 5, p. 259.

[2] *Annals and Magazine of Natural History*, 1877, p. 183.

recent times. On the other hand, the existence of the Norwegian fiords and the islands already named is evidence of an earlier subsidence of a much greater vertical range. These evidences of subsidence and upheaval go side by side almost universally, and conclusively prove the existence of a mobility in the crust of the globe, independent of lateral pressure and mountain-making.[1]

Perhaps the most interesting information of these land movements has been due to the enthusiastic labour of M. Arctowski, who, as geologist of the Belgian Antarctic Expedition, landed at twenty places on either side of the Belgica Strait, separating the Palmer Archipelago from the mass of Graham Land, and found indications in the deep valleys running down from the land below the sea-level of a general subsidence, 'the whole of the district presenting clear evidence of being a submerged region.' Thus the evidences of land movement have been literally traced from Pole to Pole.[2]

Professor George Frederick Wright, in a paper read before the Geological Society of London on 'Recent Geological Changes in Northern and Central Asia,' the outcome of a journey in 1900–1901, says, 'The Loess region of Turkestan, and indeed the whole area from the Sea of Aral to the Black Sea, appears to have been recently elevated, in some places as much as 3,000 feet.'

[1] See *Q. J. G. S.*, vol. lii., 1896, pp. 721–41.
[2] *Nature*, March 1901, p. 518.

On the other hand, the continuation of the river-valleys as submarine valleys on the Pacific submarine slope,[1] and similar phenomena at the mouth of the Mississippi and, as pointed out by Spencer, on the eastern seaboard of the United States, show that the land was at one period, in Pleistocene times, at a much greater elevation in relation to the sea than at present. Still further, Dr. Spencer has brought forward evidence of the existence of an Antillean continent in Pleistocene times which involves movements of a much more stupendous kind.[2]

In Africa, the valley of the Congo is continued seawards as a submarine valley to a profound depth; and I fully believe that a careful examination of any continent or island on the globe would yield evidences of fluctuations of level to a greater or lesser extent.

Dr. Reasch, in 'Naturen,' draws attention to the changes of level that have taken place in Iceland in recent geological times. Shallow-water molluscs are found side by side with deep-water forms. 'It was remarkable to dredge up from depths of 500 to 1,300 fathoms *Yoldia arctica*, which now lives at Spitzbergen and in the Kara

[1] To show the universality of these movements of elevation and depression, Dr. Andrew Lawson considers that there are good evidences of an uplift 'of the entire coast of California from San Francisco to San Diego, in post-Pliocene times, to an extent of from 800 to 1,500 feet' ('The Post-Pliocene Diastrophism of the Coast of Southern California,' *Bul. of the University of California*, vol. i. pp. 115-60).

[2] 'Reconstruction of the Antillean Continent,' *Bul. of the Geo. Soc. of America*, vol. vi. pp. 103 40 (1895).

Sea at depths of from 5 to 100 fathoms.' Reasch infers a sinking of the sea bottom of not less than 2,500 metres.[1]

Brögger considers that the occurrence of high-arctic fossil shallow-water mollusca of the *Yoldia* fauna at great depths in the Norwegian Sea 'is explained by the hypothesis that the sea bottom, during the time of the greatest ice-sheet of Europe, must have been uplifted at least 2,600 metres higher than it is at present.'[2] He also gives evidence of many changes of level having taken place during the glacial and post-glacial periods, and of a sinking of the land to about 240 metres, south of Mjösen.[3]

[1] *Naturen*, Dec. 13, 1900.

[2] *Norges geologiske undersögelse*, No. 31, p. 682.

[3] Dr. Hinde, in an excellent review of Brögger's 'Monograph on the Late Glacial and Post-Glacial Changes of Level in the Christiania Region,' in the *Geological Magazine* for July 1902, says: 'One of the most striking phenomena of the period of greatest submergence in the Christiania Fiord is the well-known coral reef so carefully described by M. Sars, consisting of masses of the deep-water coral *Lophohelia prolifera*, Linn., which, in a dead but well-preserved condition, occur at Drobak, south of Christiania, covering the sea bottom at levels of 60 metres below the surface, and they are also found over an area of about 100 square kilometres to a height of 30 m. above the sea. Associated with the coral is the giant form of *Lima excavata*, Fabr. Both the coral and shell are now found living in the Norwegian fiords at depths of 100-300 fathoms, and it is probable that they existed in the Christiania Fiord, at a depth not less than 150 metres, when the climate was not very different from the present and the margin of the land ice yet stood before Mjosen and Randsfiord.'

PART II

ORIGIN OF REGIONAL VERTICAL MOVEMENTS OF THE EARTH'S CRUST

While evidences of these regional movements have accumulated with the progress of geology, the attempts to explain their origin can hardly be said to have advanced at the same rate.

Incompetence of the Principle of Isostacy to explain Areal Oscillations of Level.—The principle of isostacy has been appealed to, but the mass of the solid earth involved in these movements is so vast, compared with any sedimentation that has taken place in the same period of time, that such an explanation is quite futile.

One cubic mile of sediment pressing down the earth's crust, say, 1,000 feet could not lift two cubic miles of another portion of the earth's crust 1,000 feet high, whatever mobility the undercrust may be assumed to possess. Yet I venture to affirm, if we can rely upon the observations quoted, that the movements of the earth's crust in mass in Pleistocene times have exceeded by twenty times the mass of sediment contemporaneously denuded from the land and laid down in the sea.

Many thinkers, from an early period in the study of geology, seeing that while one portion of the earth's surface has been elevated another has been depressed, have considered the phenomena to be related as cause and effect, looking upon these

opposite movements as contemporaneous; but why they should stand in this relation is not apparent. The explanation present to their minds appears to have been that in some unknown way there was a transference of material from areas of subsidence to areas of elevation, which, of course, must have taken place either in or under the earth's crust. An adequate cause of such a transference on the scale required is difficult to conceive.[1]

The additional mass of material added or pushed up in one area would have to be balanced by an additional weight added to the depressed area. A shifting of weight by denudation and sedimentation we can conceive; but, as we have seen, it is insufficient, and such transference, even if it bent down the crust, would not cause hollows or apparent depression, but rather filling-up. Where, then, can the extra weight in the depressed area be derived from to balance the extra mass in the upheaved area, for if one movement were consequent upon the other some such transference of material would seem to be required? We can scarcely appeal to secular cooling of the earth as an efficient cause, nor am I aware that any geologists have done so. It may, however, be thought that the cooling and falling in, or bending, of the crust can in some way produce this effect; but I

[1] Lyell in his *Principles of Geology* devotes chap. xxxiii. vol. ii. (tenth edition) to a discussion of many of these questions. Science has advanced since this was written, but it is well worth reading.

have a difficulty in following out such a conception. Secular refrigeration, it appears to me, would act cumulatively in one direction, whereas the movements of the earth's envelope are essentially slow pulsations.

Not due to External or Internal Transferences of Material from one Locus to another.—On a full consideration it seems highly improbable that these movements are due in any great measure to either an external or internal transference of material from one area to another. The balance of pressure must be preserved within certain limits, and I find it impossible to think of any force or agency at work in the earth's interior tending to produce such a movement; but even if there were such an agency, its effect would be limited by the possible deformation the earth could stand and retain. I venture to think that if the specific gravity of the materials of the earth were identical in each of the zones from the surface to the centre, even though the earth were as rigid as steel, the present configuration or inequalities of the levels of the earth's surface could not be retained.[1]

[1] Dr. G. H. Darwin, in a paper 'On the Stresses due to the Weight of Continents' (*Phil. Trans. of the Royal Society*, 1882), estimates that the stress-difference under the continents of Africa and America is at a maximum at more than 1,100 miles from the earth's surface, and there amounts to about 4 tons per square inch, and he remarks that marble would break under this stress, but that *strong* granite would stand (p. 229). In this calculation he appears to have halved the heights to allow for the smaller density of the surface rock.

Dr. Darwin in this paper investigates several problems of this nature, working from certain hypothetical assumptions, and the results are most interesting. A consideration of the whole subject leads me

Mean Specific Gravity of the Elevated Portions of the Crust of the Earth less than the Mean Specific Gravity of the Whole Crust.—If these conceptions have an element of truth in them, the mean specific gravity of the elevated parts of the earth, and the foundations on which they rest—that is, the continents and their mountains, and the under-mass of the earth—must be less than the specific gravity of the earth's crust and interior mass underlying the deepest depressions. So far as pendulum observations inform us, the fact appears to be established that the earth has a higher specific gravity under the oceans than it has under the continents.[1]

These observations, limited though they be, tend to show that though the levels of the earth's

to think that such stress-differences could not be maintained through geological time. We have seen that the earth is mobile and the changes it undergoes multitudinous, if slow. Readjustments are continually taking place. It is also open to doubt whether with such stress-differences a state of equilibrium sufficiently stable for the preservation of the existing inequalities of the earth's surface could exist even were the earth an inert body, which it certainly is not.

[1] See 'Results of a Transcontinental Series of Gravity Measurements,' by G. R. Putnam, with notes by G. K. Gilbert, *Phil. Soc. of Washington Bul.*, vol. xiii. pp. 31–76. See also Chap. xv., *Physics of the Earth's Crust*, second edition, where Mr. Osmond Fisher discusses the 'Revelations of the Pendulum.' Major S. G. Burrard, after a lengthened series of observations on the attractions of the Himalaya Mountains on the plumb-line, finds certain discrepancies which drive him to the conclusion that the undiscovered cause of the disturbance is traceable to a great invisible chain of excessive density, traversing India from Balasore, near the mouth of the Hooghly, to Jodhpur in Rajputana, and underlying Mandla and Bhopal, or, roughly speaking, running parallel with the Himalayan chain (*Nature*, May 1902, pp. 80–82). That there exist considerable local variations of density in the earth's crust is extremely probable.

surface are variable, there exists in the earth's interior an equality of stresses, and that the protuberances do not create stresses on their foundations tending to force them down and, by displacing the under-layers, bring about an equality of surface levels. If the protuberances—by which I mean those portions of the continents and islands that are above the mean spheroidal level—represent so much additional material piled upon a statically balanced spheroid, it seems to me that a gradual deformation must be taking place and a sinking of the higher lands. Should this be the case, what force exists within the earth to prevent the continued effects of this weighting and the natural removal of prominences above the sea-level to one uniform plane? If no such force exists, or has existed, the earth would long have ceased to possess the diversified features of land and water, so favourable to the habitation of man, and which have taken him so long to discover and map out.[1]

[1] Herschel, with great acumen, has pointed out 'that if we would divide the globe into two hemispheres, the one of which shall contain the greatest quantity of land, and the other of water, it must be cut by a plane perpendicular, not to the axis of rotation, but (singularly enough) to the diameter, passing through the south-west corner of England. The fact is instructive, as it proves the force by which the continents are sustained is one of *tumefaction*, inasmuch as it indicates a situation of the centre of gravity of the total mass of the earth somewhat eccentric relatively to that of the general figure of the external surface the eccentricity lying in the direction of our antipodes—and is therefore a proof of the comparative *lightness* of the materials of the terrestrial hemisphere' (*Physical Geography*, pp. 14 15, fifth edition). Referring to the same phenomenon, Mr. Darwin in the paper already quoted says (p. 230) it has been impossible for him in that paper 'to take any notice of the stresses produced

Internal Activities preserve the Relative Proportions of Land and Water.—Whichever way the problem is looked at, it is evident that activities must exist within the earth tending to the preservation of the relative proportions of land and water, which geologically seem to have been fairly constant throughout the ages.

We have seen that removal of weight from one locus to another by denudation, though it may have its effect combined with other causes, is, mechanically speaking, quantitatively incompetent to create the necessary movements of the crust. Nay more, in principle it contains the elements of its own destruction as a cause of variations of level of the earth's surface, for the longer the time that elapses, the less will be its effect, until its activities die out altogether. In fact the causes invoked to account for these changes of level are but secondary effects of some primary cause, which would gradually come to an end when the primary force ceased.

What, as philosophers, we have to find out is the probable nature of this active principle, which may, indeed, be reckoned as the vital force of our planet.

That upheavals above the mean level of the spheroid are balanced by depressions below it, and that they take place contemporaneously, may or may not be true. We have no solid proof that it

by the most fundamental inequality of the earth's surface, because it depends essentially on heterogeneity of density.'

is so, and nothing in disproof. If true, it would at best be no more nor less than the statement of a fact, and carry no dynamical explanation with it.

Changes of Level due to Changes of Volume in Sections of the Earth without Change of Mass.—Looking at the question from every point, the various facts already stated appear to me to point to one conclusion, and to one only. The changes of level must be due to change of bulk of certain sections or portions of the earth without change of mass.[1]

To what can we assign this active principle of change? The most obvious answer is, Variations of temperature. But what is there to create variations of temperature in the earth's mass? Blanketing by sedimentary deposits, as already explained, is quantitatively insufficient, and the movements are, as we have seen, largely independent of such surface transference of matter.

Our acquaintance with the condition of the earth's interior is so limited that little more than suggestions can be offered.

If the earth be the rigid body physicists now maintain it to be, it is obvious, considering the rate of increase of temperature downwards—the mean being estimated at 1° Fahr. per 50 feet—that it is only kept solid by compression, and that were a sufficient relief of pressure to take place at any locus the matter which before acted as a solid would then act as a fluid.

[1] Suess's views are dealt with in Chapter viii.

The intermittent action of volcanoes and the changes of position in volcanic centres that have taken place in geological time seem to point to an instability of conditions, such as would naturally characterise a globe at a high temperature kept solid by pressure.[1]

In my 'Origin of Mountain Ranges'[2] I have said: 'It seems to me that unless the matter which is molten at the surface is solid at its origin, it is impossible to formulate a satisfactory explanation of volcanic phenomena.' The view that our globe is an inert cooling mass, which suffers no change except that due to secular contraction, is not supported by the facts of geology. Indeed, the very reverse is the case.[3] Geologists are well aware that even near the surface, where the temperature is comparatively low, surprising interchanges of mineral matter have been constantly taking place. The microscopic study of rocks has revealed to us the striking fact that

[1] Sir A. Geikie, in his comprehensive work on *The Ancient Volcanoes of the British Isles*, points out that in some cases there has been a recurrence of volcanic action in certain areas through a long succession of geological periods. The counties of Devon and Cornwall are pointed to as examples. 'The extensive eruptions in Devonian time were followed, after a long interval, by a diminished series in the Carboniferous period. But the subterranean energy was not then wholly exhausted, for it showed itself on a feeble scale, in at least one limited tract of the same region, during the Permian period' (vol. ii. p. 94).

[2] P. 256.

[3] Nor, by analogy, is it supported by astronomy, for there is every reason to believe that stars do not simply decrease in temperature, but both increase and decrease (*Nature*, Nov. 12, 1896, p. 281).

chemical and mineralogical changes of a remarkable kind have taken place in the hardest rocks.

Still more striking, as upsetting our preconceived notions of the stability of matter, is the fact, determined by the experiments of Sir W. C. Roberts-Austen, that if a cylinder of solid lead is placed upon a disc of gold for a period of four years at a nearly constant temperature of 18° C. gold passes into the lead. In the lowest layer of ·75 mm. gold was present to the extent of 1 oz. 6 dwt. per ton, while in a slice 7 mm. from the surface of contact there was $1\frac{1}{2}$ dwt. per ton. It is calculated that the rate of diffusion is about $\frac{1}{350000}$ of that in molten lead.[1] In his luminous and highly philosophical address to the British Association at Glasgow, 1901, Professor Rücker, alluding to these experiments, penetratingly observed: 'The phenomena of diffusion afford conclusive proof that matter when apparently quiescent is, in fact, in a state of internal commotion. I need not recapitulate the familiar evidence to prove that gases and many liquids, when placed in communication, interpenetrate or diffuse into each other; or that air in contact with a surface of water gradually becomes laden with water-vapour, while the atmospheric gases in turn mingle with the water. Such phenomena are not exhibited by liquids and gases alone, or by solids at high temperatures only. Sir W. Roberts-Austen

[1] *American Journal of Science*, March 1901, p. 236, quoted from *Journal of Chem. Soc.*, lxxx. ii. 9.

has placed pieces of gold and lead in contact at a temperature of 18° C. After four years the gold had travelled into the lead to such an extent that not only were the two metals united, but on analysis appreciable quantities of the gold were detected, even at a distance of more than 5 millimetres from the common surface, while within a distance of three-quarters of a millimetre from the surface gold had penetrated into the lead to the extent of 1 oz. 6 dwt. per ton, an amount which could have been profitably extracted.'

If interchange of matter takes place under these apparently unlikely conditions, how much more intense must be the interaction of the elements in the lithosphere of the earth! Rocks formerly at the surface, whether originating from sub-aërial waste and sedimentation or ejected from volcanoes in the form of lava or ashes, become buried under other matter and are subjected to pressure and heat. The materials of our planet, so far as we can observe them, are in a constant state of flux and change, sometimes very actively, at other times more slowly, and many are the changes rung on the combinations of the elements. These surface changes in the composition and form of the envelope of the earth are the life of the planet, and a continuation of perhaps greater changes which they have formerly undergone in the earth's interior.[1]

[1] Since these and following lines were written Lieut.-General McMahon, in his address to Section C of the Belfast meeting of the

When we see that the outer envelope, with which we are best acquainted, undergoes an evolution through the continual addition made to its sedimentary crust by the waste from rocks ejected at the surface by volcanic action, and by the chemical and mineralogical combinations and recombinations that take place from their new associations, can we refuse to believe that a mass of complex matter in the earth's interior, vast in comparison, at a very high temperature and *under enormous pressure, varying with its depth*, is not similarly, but to a higher degree, subject to change and interchange, combination and recombination?

Furthermore, as showing the complexity of these molecular actions, it was pointed out as early as 1825, by Scrope, in his classic work on volcanoes, that there is good reason to believe 'that in the greater number of cases lava, when issuing from a volcanic vent, is already granulated; or composed of more or less imperfect crystals enveloped in a base or paste of finer grain,' and that its fluidity is due to contained water or vapour of water.[1] These crystals appear to have 'suffered considerable

British Association (1902) on 'Rock Metamorphism,' has stated that a rise of temperature 'increases the porosity of minerals and facilitates the passage of liquids and gases through their pores.' He instances the well-known fact that metamorphic changes sometimes begin at the heart of a crystal, and leave the peripheral portions of it fresh and unaltered. 'In such cases the chemical agents of change have evidently passed freely through the outer parts of the crystal, and have by preference selected its internal parts for attack.' The address is an interesting and luminous exposition of the interpenetrability of matter.

[1] Second edition, pp. 116–17.

attrition, rounding, and disintegration through mutual friction.' Judd adopts this view, adding some confirmatory microscopic evidence in his 'Volcanoes, What they are and What they teach.'[1]

Evidence of Volcanoes as to the Interior of the Earth.—It is well known that the lavas ejected from volcanoes differ at various times. At one time basic lavas prevail, at another acidic.[2]

It has been sought, by Richthofen and others, to educe laws which govern the succession of these eruptions, but, according to Judd, such attempts have hitherto failed.

The fact, however, of the variability of the ejections points to mineral and chemical changes going on in nature's laboratory. Professor Iddings has lately, in a paper published in the 'Quarterly Journal of the Geological Society,' 'On Extrusive and Intrusive Igneous Rocks as a Product of Magmatic Differentiation,'[3] sought to prove that 'in a region of eruptive activity the succession of eruptions commences in general with magmas representing a mean composition, and ends with those of extreme composition.' Professor Iddings considers that differentiation in the most deeply seated magmas progresses the most slowly, other things being equal, and that the depths at

[1] 1881, pp. 60-61.

[2] It would appear that acid lavas usually precede basic, and that not infrequently a lava of intermediate composition is the first ejected. See *Volcanoes*, by T. G. Bonney, p. 305.

[3] Vol. lii. p. 606, 1896.

which the once molten floods of rhyolite and basalt of the Teton range were differentiated must have been profound. One cannot help assenting to this proposition, when it is considered that the whole mass of the volcanic matter extruded in the area treated of, which includes the Yellowstone National Park, is estimated at 4,000 cubic miles.[1]

Iddings also infers that the process of differentiation is influenced by 'the size, position, and shape of the reservoir, and possible differences of temperature; the times of eruption, size and shape of the conduit, and the laws controlling the flow of liquids.'[2] From this it would seem that, in his opinion, mechanical conditions alone may be a potent influencing agency. If, however, changes in the magma can be induced by mechanical causes, the resultant differentiation will bring chemical forces into play; indeed it is difficult to see where action and reaction would cease. When we consider that even in minerals that crystallise out in two systems the specific gravities are seldom the same, though the chemical constituents are identical, it would seem that such differentiation, whether proceeding from separation or recombination of the elements, would be accompanied by a change of bulk. Furthermore, when new combinations take place, they are usually accompanied by a change of temperature, which must affect the bulk of the matter in which it occurs as well as that surround-

[1] P. 611. [2] P. 616.

ing and above it, through which the change of temperature must travel.[1]

Recalescence.—Even in the process of cooling sudden changes of temperature take place, as instanced in what is called 'recalescence.' When a bar of hard iron is cooled from a white heat, there is a sudden development of heat at a dull redness and the magnetic properties of the iron change abruptly.[2]

It was shown by Osmond that as a mass of iron or steel cools down there are at least two distinct evolutions of heat, one occurring at a variable tem-

[1] In a carefully thought-out treatise of much originality on 'Metamorphism of Rocks and Rock Flowage' (*Bulletin of the Geo. Soc. of America*, vol. ix. pp. 269 328, 1898), Van Hise shows in considerable detail that these mineral changes often result in increase of bulk and evolution of heat; in other cases in decrease of bulk and absorption of heat. He also points out that pressure and temperature are main factors in these mineralogical transformations. Finally, he comes to the conclusion that there exist in the earth's crust two 'physico-chemical zones,' an 'upper' and a 'lower.' 'In the upper of these the reactions take place with the expansion of volume and with the liberation of heat as end results. In the lower the reactions take place with contraction of volume and with the absorption of heat as end results. Some of the more important reactions in the upper zone are hydration, oxidation, and carbonation; some of the more important reactions in the lower zone are dehydration, sulphidation, and silication' (p. 328).

Without committing oneself entirely to so wide a generalisation, the facts brought forward by Van Hise abundantly justify the contention of this treatise, that to a very considerable depth the materials of the earth's crust are in a continual state of flux and reflux. It is, however, at depths considerably greater than those treated of by Van Hise, and affecting far larger masses of the earth, that I seek to show that changes of volume slowly take place which distort the symmetrical figure of the spheroid, producing pulsations of considerable amplitude, which are disclosed to us by the oscillations of the land, sometimes affecting large continental areas.

[2] *Engineer*, July 8, 1887, p. 25.

perature not higher than 855° C., the other at a more constant temperature near 650° C.[1]

Just as a certain temperature must be attained before any particular combination can occur, so there is a certain temperature above which any particular compound cannot exist.[2]

That the phenomenon of 'recalescence' is not only accompanied by a sudden development of heat, but also by expansion and change of bulk, is shown by a process in certain American rolling-mills, where the bars are rolled in lengths of 300 feet, and allowed to cool in a special cooling bed, which keeps them straight. The great length of the bars renders very evident certain peculiarities in their contraction when cooling, the total movement being between 3 and 4 inches. It is noted that when first placed on the bed contraction proceeds rapidly, then checks, and finally ceases. The bar then expands again, probably during recalescence, after which the contraction is uniform till the metal is cold.[3]

Ordinary chemical reaction may be modified by the abnormally high temperature attained during the Bessemer blow.[4] H. Le Chatelier has shown[5] that if a small quantity of clay is heated

[1] *Nature*, March 6, 1890, p. 420. Sir W. Roberts-Austen says: 'We now know that as steel cools down there may be at least six points at which molecular change occurs, accompanied with evolution of heat' (*ibid.*, May 11, 1899, p. 43).

[2] *Nature*, Dec. 18, 1890, p. 166. 'Chemical Action and the Conservation of Energy.' Spencer U. Pickering.

[3] *British Architect*, March 23, 1900, p. 201.

[4] *Nature*, Nov. 20, 1890, p. 52.

[5] *Proc. of Inst. of C.E.*, vol. xci. p. 468 (1887 88).

rapidly, a retardation of the rise of temperature is observed at the moment of dehydration, and the points at which these retardations take place appear to be characteristic of differences in the several hydrated silicates of alumina. The result is similar to that which has already been determined in lime burning, and is due to the circumstance that the speed of chemical reaction, as soon as it has attained a suitable value, is enormously augmented for very small increments of temperature. Instances such as these could, no doubt, be multiplied almost indefinitely by those better acquainted with chemistry than I am.

Mr. George F. Becker, in a paper entitled 'A New Law of Thermo-Chemistry,'[1] says: 'There is every theoretical and experimental reason to suppose that the fluid eruptive magma consists of one or more compounds differing essentially from the minerals eventually found in it. In cooling it must, therefore, undergo a series of chemical and physical changes. The formation of any new stable compound, whether fluid or solid, in the mass converts other forms of energy into heat; but at the same time the subtraction of any such group of molecules from a previously existing combination alters the chemical constitution of the residue.'

Change of Volume with Change of Conditions. Perhaps the most singular instances of change of physical properties and of volume with change of conditions are those mentioned by Dr. John

[1] *Amer. Journ. of Science*, Feb. 1886, pp. 120-25.

Hopkinson in the discussion of Mr. H. F. Parshall's paper, read before the Institution of Civil Engineers, on the 'Magnetic Testing and Data of Iron and Steel.'[1] Nickel-steel containing 5 per cent. of nickel possessed very high tensile strength and a very high maximum magnetisation.

The same was the case as regards magnetisation, to an even higher degree, with a mixture in which 70 to 73 per cent. was nickel and the remainder iron. More remarkable, however, is the fact that the nickel steel containing an intermediate percentage, such as 20 or 25 per cent. of nickel, was as delivered from the manufactory practically non-magnetic—almost as non-magnetisable as manganese steel. It appeared to be a material which presented none of the marked peculiar characters of nickel or iron with regard to magnetism. Nevertheless, if a sample of this material were cooled to 20 C. or 30° C., or, even better, a lower temperature, it would be found that it presently became strongly magnetisable. But that is not all. If the material were allowed to become warm again, it remained magnetisable, and continued to be so until heated to about 600° C. or 700° C., when it changed over again and became non-magnetisable, and when cooled to the ordinary temperature remained so. Many other of its physical properties were also changed. Its tensile strength when in its non-magnetisable condition was 50 tons to the square inch, and the breaking stress was increased to

[1] *Proc. of Inst. of C.E.*, vol. cxxvi. p. 253 (1896).

80 tons per square inch when the metal was cooled and rendered magnetisable. The extension in one case might be 30 per cent.; in the others it fell to 7 or 8 per cent. *Finally, the density in the non-magnetisable state differed by about 2 per cent. from the density in the magnetisable condition.*[1]

Carbon in Steel subjected to great Pressure changes to Graphite or Diamond.—It was also stated by Mr. Parshall in his paper[2] that the small amount of carbon present in cast steel was in the combined state, but in castings subjected to great strains the combined carbon partly changes to graphite.

In a paper by Leon Franck, of which an abstract appears in the same volume,[3] it is stated that carbon in the diamond form was obtained by Moissan, in 1894, by chill casting at a very high temperature, when the interior solidifies under the pressure of a rigid envelope.

Furthermore, M. Franck himself obtained from melted but unwrought steel, by treatment with acid, particles that have the characteristic octahedral form of the diamond, but so small as to require an amplification of 2,500 to 3,000 to render them visible. Unfortunately, our knowledge of the chemical reactions and dissociations that take

[1] It is a still more curious fact, noted by Guillaume, that there are 'certain nickel steels that will not expand by heat, and others that contract when heated and expand when cool.' Sir W. Roberts-Austen's Presidential Address to the Iron and Steel Institute (*Nature*, May 11, 1899, p. 42).

[2] *Proc. of Inst. of C.E.* vol. cxxvi. p. 228. [3] *Ibid.* p. 478.

place at high temperatures in large masses has to be almost entirely drawn from the processes of iron manufacture; yet, as showing how minute quantities of matter may alter the physical conditions and melting-points of other matters, I may mention that the presence of 0·5 per cent. of carbon lowers the melting-point of iron from 1,600° C. to 1,530° C.,[1] and it is a known fact that endothermic combinations take place at a high temperature.[2]

The Earth not an Inert Mass cooling in Space. Though, as admitted, our knowledge of the earth's interior is so extremely limited, I have, I trust, brought forward sufficient reasons to show that whatever conditions obtain, they are of considerable complexity, and that the conception of the earth simply as an *inert* mass cooling in space is a fallacious one. Cooling in space it certainly is; but the phenomena of volcanoes, with their varying products of emission at varying temperatures, the mineral changes that we see have taken place and are still taking place in the rocks within our ken, the phenomena of mineral veins and earthquakes all point to the existence of living forces which analogy leads us to interpret as evidences of chemical changes and variations of temperature, accompanied by changes of volume in the materials of which our planet is composed.

If there be any truth in these views, we may naturally look to the *loci* of volcanic energies as

[1] *Nature*, May 11, 1899, p. 42. [2] *Ibid.* May 11, 1899, p. 41.

areas in which these chemical and thermal perturbations are exceptionally active.

I have in my 'Origin of Mountain Ranges' given reasons for thinking that the volcanic pipes from which these lava emissions proceed are in communication, either direct or indirect, with the interior heated nucleus. If this be so, every emission of lava will be accompanied by a movement of the heated matter of the earth's interior towards the base of the vents. Such a movement will necessarily bring together matter differing somewhat both in constitution and thermal condition. From these, other combinations and reactions will follow, accompanied by the disengagement of heat and alteration of volume. Volcanoes may be taken as indicative of the actual existence of these conditions, and it is in volcanic areas that the greatest recent regional changes of level have taken place. From the extreme south of South America to the extreme north of North America, in Alaska, a distance of eight thousand miles, evidences of great changes of level have been recorded, as already mentioned, by Charles Darwin,[1] Dr. G. Dawson, Israel Russell, and others, amounting to as much as 5,000 feet, and this line is the seat of numerous active volcanoes.

The Antilles may be considered to belong to the same system, and to be connected with the volcanoes of Central America; and, as already

[1] *Geological Observations.*

stated, it is here that Dr. J. W. Spencer finds evidence of the former existence of a Pleistocene continent. These instances may be multiplied by examples from other parts of the world.

Part III

MAGNITUDE OF THE MECHANICAL FORCES INVOLVED IN CONTINENTAL OSCILLATIONS

Having now established the existence of these widespread movements affecting enormous areas of the earth's lithosphere, it will be well to consider what relation in magnitude they bear to the other geological agencies, mostly sedimentary, at work in building up the complex features of the earth's crust. A little consideration will serve to show that these secular movements of elevation and depression involve the movement of sections of the earth's crust enormously greater than the mass of any known group of sedimentary deposits.

That this is a necessity for the renovation of the earth's surface will also become apparent.

Let us begin with the simplest case that can be assumed—that of a continent which remains a continent till it is planed down to sea-level by sub-aërial agencies. The mechanical detritus worn from the land by sub-aërial agencies will be laid down as a fringe to the shores of the continent; the soluble matters will be more widely spread. But for the sake of simplicity let us assume these sediments to fill up the adjoining seas to sea-level.

Now, it is a necessary condition for the retention of land areas that these fringing deposits should in their turn be elevated above the sea-level and made into dry land, and that the continental area should sink below the waves to again receive sediment. The minimum displacement of mass required to effect this object would be, as regards upward movement, the elevation of an equivalent amount of sediment above the sea-level; that is, mass for mass. If the area and thickness of the sedimentary deposits were the same as the area and thickness of the mass denuded from the continent, the mechanical work involved in elevating the sedimentary deposits so that their base should be at sea-level would be, if we leave out of account the mechanical work involved in lifting the platform on which they rest, exactly equivalent to the mechanical work previously done in the gravitation of the sediment from the continent to below sea-level.

Nature's machinery for lifting these sediments for the manufacture of dry land, and their replacement in continental masses, is not, however, of this economical character.

To lift the sediments back again to their original position means also an upward movement of the *platform on which they stand*, and the energy consumed in this work would be far in excess of that consumed in the lifting of the sediments alone. The amount of energy consumed is dependent upon the mode in which the raising of the

platform comes about; but in any conceivable case the internal work in the earth's lithosphere and below it must be very great.

Furthermore, the lifting of these sediments will involve the lifting of a much larger area of the earth's surface than the sediments actually cover. Not only so, but the origination and preservation of the geographical features of the earth require, as proved by geology, frequent fluctuations of the level of the land, involving oscillatory movements of the earth's crust of vast extent and considerable vertical range.

Nature does not in this case appear to work with a parsimony of energy, and the internal forces required for the maintenance of the external features of our planet are indeed active and great.

Generalisations

A careful consideration of the bearing of the facts I have stated seems to lead to the conclusion that the irregularities in the form of the spheroid, which give us our diversified world of land and water, arise from differences in the specific gravities of sections of the earth's crust and the underlying matter.

These specific gravities are not stable, but are subject to slow changes consequent upon changes of temperature. A rise of temperature and local increase of volume create protuberances which may be of continental extent. A fall of tempera-

ture and decrease of volume leads to depressions which may culminate in those profound abysses of the ocean aptly named 'deeps.'

Thus it follows that these departures from the regular spheroidal form are not original and permanent; nor are they features that have been growing larger from the dawn of geological history, such as would be likely to occur from a differential radial shrinkage of the earth.

That these fluctuations of thermal conditions actually take place is evidenced on a small scale by the thermal fluctuations known to occur in volcanic emissions, and by the varying composition, specific gravities, and temperatures of the lavas from time to time emitted from the vents. The motive force expelling the volcanic products is due largely to increase of temperature, and consequent increase of volume. For every relief of such expansion of volume by emission, thermo-dynamic laws show us that a reduction of temperature and shrinkage must take place in the supplying reservoir. Hence result the periods of quiescence which follow energetic volcanic action, lasting until the molten matter of the reservoir becomes again raised to the necessary temperature to burst its bonds.[1]

[1] In the discussions that have taken place on volcanic action called forth by the recent activity of Mont Pelée it has been suggested by several geologists that the cause of the recent outbreak is the development of an immense earth-fold in the West Indian area. I cannot but express my dissent from this dictum. In the first place, where are the proofs that such an earth-fold exists and is undergoing further development? The connection between earth-folding and volcanism as cause and effect is not supported by geological evidence. On the

Concluding Observations

Fluctuations of the level of the land, but more especially of the sea-bed, affect the relative levels of land and sea in an indirect manner. Thus, for instance, if one of the oceanic 'deeps' was to be obliterated by a volume-expansion of the underlying matter of the earth, it would cause a general rise of the sea-level and a transgression of the waters over the lower-lying lands. This rise and redistribution of the fluid envelope would be universal, and seems to give a clue to the curious fact I have often pointed out, that the beds of all great continental rivers—as proved in many cases by borings, and in others by the extension of their valleys across the continental shelves—lie at a lower level now than they have done in some former periods of their history. They are, in fact, drowned valleys, or, if not actually submerged, the sea is excluded by the deposits filling up the river-bed.

If we admit that the fluctuations of the relative levels of land and sea are due in the first instance to positive movements of the solid land—and of this I have no reasonable doubt—it appears to me that such movements can only be explained in one of these two ways, viz. (*a*) By actual trans-

contrary, it is most remarkable how distinct the phenomena seem to be, for in great folded mountain ranges like the Appalachians we find symmetry of curvature in the appressed beds, instead of the mixed mass of folds and igneous rocks we should expect were the development of folding the cause of volcanic activity.

ference of material from one locality of the spheroid to another; or (*b*) by increase or decrease taking place in the specific gravities of the rocks or materials of the earth under certain *loci* of the spheroid, which *loci* change from time to time.

Explanation (*a*) seems extremely improbable, especially if we attempt to apply it to regional movements on an extensive scale. In addition to there being no known cause sufficient to produce such a displacement of matter, it is more than questionable whether the equilibrium of the spheroid could be maintained under such a vast redistribution of weight. In mountain-building, a change in the loading of the crust takes place by lateral creep and on a much smaller scale, and the movement is balanced by other internal forces.

Explanation (*b*) has been discussed and expounded in previous pages. It appears to me to invoke the only cause competent to account for these vast, though slow, movements, and while providing the necessary energy, preserves throughout geological history the equilibrium of the spheroid.

Statement of the Theory of Geomorphic Changes

1. The earth is an oblate ellipsoid of revolution, the solid materials of which it is constituted being arranged in the following order:

a. An outer crust, shell, or lithosphere, consisting of sedimentary, igneous and metamorphic

rocks, having an approximate depth of thirty miles.

b. A semi-plastic underlying shell, or igneous magma, potentially containing minerals such as are associated with volcanic action at the present time, and are found constituting rocks intrusive and consolidated in the crust (*a*). These rocks include granite, diorite, dolorite, basalt, and innumerable allied species. The thickness of this shell is unknown.

c. An immense interior spheroid, usually called the 'nucleus,' forming the main mass of the earth, the lithosphere being relatively a surface film.[1]

2. The main spheroid consists of matter and minerals, varying in specific gravity, the heaviest of which are supposed to be nearest the centre. Whether this be so or not, there can be little doubt that the density of the matter, which increases with depth, is largely due to the enormous pressure to which it is subjected.[2]

3. The temperature of the earth increases with depth, so far as all our limited observations enable us to judge. Generalising from temperatures taken in mines, borings, and tunnels, the average is generally assumed to be about 1 degree Fahrenheit per 50 feet. No observations have ever, to

[1] The term 'nucleus,' I fear, is responsible for many misconceptions of a far-reaching nature hinging upon the relative proportion of the 'nucleus' and its envelope.

[2] In this connection see 'A Theory to account for the Airless and Waterless Condition of the Moon,' by Fred F. Grensted, M.A. (*Proc. Liverpool Geo. Soc.*, Sess. 1887 88).

my knowledge, disclosed a temperature gradient decreasing with depth. Portions of the crust are locally heated, and others locally cooled. If temperature gradients could be made representing the heating of the crust in several *loci* to a depth of thirty miles, great variations would doubtless show themselves; and, indeed, molten matter might be tapped at different levels.

4. There is good reason to believe that this rise of temperature with depth does not go on indefinitely, but at a depth of several hundred miles shades off into a more uniformly heated central mass, which may properly be called the nucleus, and which exists at a very high temperature.

5. The earth as a whole is pronounced by Lord Kelvin and George Darwin to be at least as rigid as steel, and by Mr. John Milne to be twice as rigid as steel. The fact that earthquake waves are propagated more rapidly from the Antipodes than from intermediate places seems unaccountable except on the hypothesis of the earth's interior rigidity—the waves travelling along the diameter in one case, and in the other along parts of the circumference where the rocks are less rigid and more or less discontinuous.

6. If the preceding conclusion (5) be right, it follows that, the earth having an enormous interior temperature, its rigidity can only be the result of pressure due to the gravitation of the materials towards the centre. It also follows, if the main mass or interior spheroid of the earth is of a

uniform temperature, that its rigidity must increase as the centre is approached.

7. While chemical action is modified, and in some cases annulled, by high temperature, it is increased by pressure. From this it naturally follows that if the temperature of the interior mass is high enough to create complete dissociation were it transferred to the surface, the pressure which is sufficient to counteract expansion and keep all the particles in solid apposition must profoundly affect the chemical interactions.

8. The continents, together with some of the submerged masses of the earth proved by soundings to exist, are protuberances on the true spheroid, due to their inferior density and greater volume. The 'deeps' of the ocean are depressions below the true spheroid, due to the superior density and less volume of the underlying masses of the earth.

9. The volumes and the specific gravities of the constituent minerals are subject to increase and decrease, consequent upon internal changes of temperature and chemical interactions, as explained in detail in previous pages. Expansion increases the volume of the mass affected, and leads to an elevation of the earth's surface, while contraction decreases the volume and ends in depression. The *loci* of these opposite actions change with the sequence of the ages. In this way continental movements involve but slight changes in the loading of the earth, other than those treated of in

10 and 11, the equilibrium of the spheroid being subject to no sharp disturbance.

10. Increase of volume by expansion, which leads to these continental or epirogenic uplifts, must not be confused with the expansions and contractions to which mountain-building is due. These are mostly lateral and intermittent, creating creeps of the lithosphere and surface rocks, ending in the folding and permanent ridging-up and corrugations of the earth's surface. They cause a lateral transfer of material from one *locus* to another. Denudation and sedimentation also vary the loading of the earth's crust; but all these mechanical agents, though great, are small in comparison with the energies which uplift continents with mountain ranges piled thereon. These facts will become more evident in the next chapter.

11. A variation of the loading of the earth's crust consequent upon these movements, and one of universal effect, remains to be stated. It is this: no alteration in the model of the surface of the lithosphere where it is covered by ocean waters can take place without the waters being more or less displaced. We have seen that a depression of the ocean bottom will draw the waters from the land and increase the land areas, while a rise of the sea-bed will cause a transgression of the oceanic water over the land. A continental rise or alteration of the relative levels of land and sea will have the same effect, as the emerging land will displace an equivalent amount of water. A depression of

the land will be followed by a transgression of the waters over the sinking surface.[1]

As, according to this theory, these elevations and depressions result from alterations of volume, unaccompanied, except to a minor extent, by lateral movement of mass or increase of relative weight, a transfer of the ocean waters to a depression would increase the weight upon that portion of the earth's crust and emphasise the depression, while a rise of the sea-bed would decrease the weight upon that area of the crust and increase the elevation. To every movement of elevation and depression the mobile oceanic waters would respond, intensifying the dynamical effects originating in the pent-up forces of an intensely heated globe.[2]

[1] Suess has calculated that the sinking of the Black Sea and Ægean areas—which he considers to be of quite recent date—would, if no water previously occupied these areas, lower the sea-level all the world over to the extent of about 4 metres. An independent calculation leads me to think that the volumes are over-estimated. The Black Sea contains about 77,602 cubic geographical miles of water, which is equal to a lowering of the sea-level by about ·716 of a fathom, or 1·31 metre. Probably with the Ægean area the lowering might reach 2 metres.

[2] G. H. Darwin, in his most interesting work on the Tides, says, 'When the barometer is very high we are at least 3 inches nearer the earth's centre than when it is very low' (p. 133).

CHAPTER II

CONTINENTAL EVOLUTION AND ITS RELATION TO MOUNTAIN BUILDING

CONTINENTS *outlined by Mountain Chains.* Those who have studied the 'Origin of Mountain Ranges' will, perhaps, ask in what way these deep-seated automatic changes in the volume of sections of the globe will affect the lateral expansions, to which I attribute the folding and building up of mountain chains.

It is evident that these epirogenic movements are to a large extent independent of sedimentation, for, as we have seen, great movements of considerable vertical amplitude are found to have taken place, and are registered, on all continents and most islands, by raised beaches, and by the beds of rivers continued far out into the ocean at depths that preclude the possibility of their having been excavated under present conditions. From these and other facts we infer that these trenchings of the continental slope are due to sub-aërial denudation, and are what American geologists call 'drowned valleys.'

It is also evident that not only are the portions of continents which lie at the lower levels, and con-

stitute the greater areas, subject to these periodical movements, but the mountain ranges built up by lateral expansion of the strata are affected by the same deep-seated voluminal expansions or contractions, and partake of the vertical rise or fall of the continents on which they stand.

It is to these two forces acting in unison that the continents owe their existence.

Long-continued denudation of land areas provides the material out of which mountain chains are built up. These sediments, it has been shown, are deposited mainly on areas in the oceans of less extent than the land areas from whence they are derived, the heavier and coarser grained nearer to land, the finer further removed, while the matter held in solution is of almost world-wide distribution.

Out of these abnormally thick deposits, by expansion and folding in a manner fully explained in the 'Origin of Mountain Ranges,' the massive sedimentary beds are condensed and forced up into mountain chains.

These mountain chains sketch out the extension of continental land, and although pulsations of the crust may occur without any apparent direct connection with sedimentation as a whole, the two forces—the one deep-seated, the other more superficial—act in unison, and maintain continental protuberances as permanent features of the globe.

The great land areas are of less mean specific gravity than the strata of the crust submerged by the deep seas. This is owing to the various causes

which increase the relative volume of the continental rocks and underlying sections of the nucleus of the globe, as sketched out in Chapter I.

It is also worthy of remark that the *sedimentary* rocks are, as a rule, of less specific gravity than the *igneous* rocks, from which they have originally been derived. This will to a certain extent, by a balancing of the earth's masses, tend to create permanent protuberances above the mean level of the spheroid.

The mountain chains sketch out the borders of the continents for long periods, as is seen in the western chain of South and North America. Older mountain masses or chains may define the borders elsewhere, as do the Appalachians in North America and the mountains of Brazil in South America; or, in Europe, the region of Scandinavia, generally recognised as a very old land mass.

These mountain chains, both old and new, have their main axes at various angles to each other; and it follows from this that when subsidence takes place the normal angular outlines of the continents must, consequently, follow.

This is the simple explanation of a geographic form that has seemingly proved a matter of mystery to many minds.[1]

Continents now at Low Levels.—It is a remarkable fact that, in every known instance where

[1] Lord Avebury, in his *Scenery of England and the Causes to which it is due*, gives a similar explanation of the tendency of so many great masses of land to point southwards (p. 501, 1st edition).

proof is possible, the continents are at lower levels now with respect to the sea than they were on some former occasions during their lengthened history. That is to say, none of the continents are now at their maximum height above the sea-level. I have pointed this out elsewhere.[1]

To what is this striking phenomenon attributable?

Is it that the geographic forms of the spheroid are in less relief now than formerly—that the continental plateaux have subsided, or that the sea and ocean beds have risen? It is difficult to believe that the continental movements can have been coincident, and universally in one direction all over the globe. It may be that in the vertical slow oscillations to which, as we have seen, the whole crust is subject, one land area has moved vertically upwards while other areas subsided, and it happens that none of the continents are now at their maximum elevation. The remarkable fact that the borders of all the continents, where sufficient soundings have been taken, show undoubted indications of having been formed by sub-aërial denudation, is a proof that at least portions of the land constituting the present continents have been in times past at a much greater elevation above the sea-level than they are now.

Geographic Relief of the Globe due to two Causes.—It would appear, then, from all the fore-

[1] 'Rivers,' *Transactions of the Liverpool Geological Association* vol. ii.

going considerations that the geographic relief of the globe in its larger features is due to two causes :—

Firstly, to differential alterations of volume in large sections of the globe, which take place with extreme slowness and, being deep-seated, upheave or depress portions of the earth's surface-crust without wrinkling it.

Secondly, to the tangential creep and ridging up arising from the heating of sediments and variations of temperature, and consequent expansion, within the earth's crust brought about by sedimentary deposition, as fully explained in the 'Origin of Mountain Ranges' and in Chapter IX. of this work.

In the first case there is change of volume without local transfer of material; in the second there is actual movement of material laterally, and a building up of permanent ridges or wrinkles, which constitute the most striking geographic forms to be seen on the earth.

Finally, it must not be lost sight of that the first of these movements affects vast masses of matter, and I think that any one who will carefully weigh the question will admit that, *a priori*, it is mechanically improbable that this matter can have been moved from one *locus* to another, especially when it is so difficult to conceive an adequate explanation of such an hypothesis.

Permanence due to Vastness of the Masses affected.—It is to the vastness of the masses affected that we owe the comparative permanence

of the geographic forms. The agencies of change, which have been fully discussed in Chapter I., cannot but act slowly; accumulating stresses may take place, and it is conceivable that catastrophes might occur; but there is really no evidence of them in the earth's geologic history, except on a minor scale.

I have shown that sedimentation and mountain building are in the nature of cause and effect. Whatever be the fluctuations in the rate of denudation and deposition, such fluctuations are limited by meteorological conditions; and even if the largest coefficient of denudation be granted, the time taken up in providing materials for the evolution of a mountain chain must be enormous.

Therefore it appears that, the evolution of a continent being dependent upon two factors firstly, the alteration in volume of enormous sections of the earth's mass, and, secondly, the deposition of sediment and its lateral folding into bordering mountain chains the rate of continent-making must necessarily be extremely slow. To this is attributable the comparative permanence of land conditions, which has led some naturalists to infer that continents have, speaking generally, been in the same relative position throughout all geologic time.

CHAPTER III

CONTINENTAL GROWTH AND GEOLOGICAL PERIODS[1]

IN the early days of geology a period indicated a certain section of the earth's history distinctly marked off from that which preceded and that which followed. Each period had a fauna and flora peculiar to itself, by which it could be recognised through the fossil remains found embedded in its rocks. There is no doubt that the Mosaic account of the Creation gave traditional support to the notion of distinct breaks in the geological chain. Each period represented, not unnaturally, to early thinkers a separate creation, followed by complete destruction. What first attracted attention were the salient differences between the fossil contents of strata geologically far apart, such as the reptiles of the Lias and the fishes of the Chalk. Ingenious men found then, as Mr. Gladstone did later, a parallelism in the order of creation between the Mosaic account and the record of the rocks.

When, however, the record was further searched, interesting links were discovered, which, if they did

[1] This chapter is the reproduction of an article that I contributed to *Natural Science* in 1894.

not actually bridge over the differences, led men to think that, could all be restored, the earth's history would be found to be one continuous record, unbroken by cataclysmic collapses and successive repairs. Curiously enough, the cataclysmal ideas held their ground in face of the uniformitarian theory of the earth given to the world by Hutton at the close of the last century. Lyell, following on the same lines as Hutton, with a wealth of illustration and rare literary skill, showed that a true interpretation of Nature in the past was to be sought in the action of present causes. The science of geology was thus put on a stable base, and men were taught to arrive at their opinions by reasoning upon facts, instead of merely giving free scope to their imaginations. The doctrine of uniformity may not be theoretically correct; indeed, uniformitarianism is a misnomer, even as applied to Lyell's conception of the history of the earth, which might, with quite as much justice, be called development. The popular conception, however, of uniformity was that things are now as they have been in the past and as they will be in the future. From this it resulted that 'periods' came to be looked upon only as arbitrary divisions of geological history, which enabled one to grasp the sequence of geological events. Indeed, it is but fair to say that there is not in the whole of Lyell's 'Principles,' for which no one has a profounder regard than myself, any indication of how the distinctive geological and physical features of the

several periods came about, or, indeed, why they should exist at all. As a student of Lyell from my earliest dabblings in geology, this was long a mystery to me. In putting forward the following suggestions as to how the periods were evolved, I do so with all humility, looking upon them as a development of the great master's work.

Sedimentation and Land-making.—It is a well-known axiom in geology that the land is being lowered at an average rate of about 1 foot in 4,000 years by meteoric action, by rain and rivers, or all those chemical and mechanical forces that come under the general term sub-aërial denudation. To this is to be added the mechanical abrasion of coasts. The matter carried into the ocean in solution in river waters is, I have shown, on the average of many years, about one-third that in suspension. This seems at first blush a large proportion, but when we consider that the matter in solution is a much more constant quantity than the matter in suspension, our surprise is modified.

The sedimentary matter—which is chiefly silica, either in the form of grains of sand or in a much finer state of comminution, as flour of rock mixed with the decomposition products of various rocks, notably felspathic, forming what when deposited we call clay—is laid down in a more restricted area than the matter in solution. Mixed with these products of denudation are calcareous particles, mica, and other minerals, which all go to make up one or other of the various sedi-

mentary strata. These are, as a whole, often looked upon as the effects of mechanical erosion, but, so far as this is an expression of dynamic action, they are only partially so. Chemical forces have, in my opinion, much more to do with loosening the bonds of the rocky particles than has mere pounding of the boulders along shores and river-beds; they also effect the separation of the rock-masses. To be impressed with this fact one has only to look at some of the enormous masses of rock in mountain districts in Wales, moved to their present positions during the last phase of the Ice Age. Although they have been exposed to nothing more than meteorological influences since, they are frequently split up into many separate blocks, and are much weathered. Likewise, in granite districts enormous masses of granitic sand are, as we may say, liberated by the decomposition of granite. To these sediments must be added boulders and pebbles which go to form conglomerates. Boulders are, however, seldom delivered into the ocean by large rivers. They remain in the higher reaches or mountain tributaries, so that the boulder-beds found in the sea are either the products of coast erosion, the dynamic undermining of cliffs by wave action assisted by meteorological influences, or are formed by mountain torrents with swift gradients, or are carried by glacial agency or by floating ice.

These mechanical products of denudation are sifted out and arranged by the ocean waves, tides,

and currents, in the order of their sizes and specific gravities, so that the boulder-beds are mostly beach deposits, while the pebbles, gravel, grains of sand, and finely triturated material are carried out farther and farther from the coast and into deeper water in inverse proportion to the size of the particles.

It is well known that the thickest deposits accumulate nearest the land masses. I do not, indeed, think that any fixed line or margin can be drawn, within which limit deposits may be considered terrigenous and outside oceanic, but it is a general principle to be kept in mind. When coast waters are shallow, and what is called the continental sub-aqueous plateau extends far out from land, then these mechanical sediments will under ordinary circumstances extend the farthest.[1] But in the extremely interesting 'Reports on the Dredging Operations off the West Coast of Central America to the Galapagos, to the West Coast of Mexico and the Gulf of California,'[2] by the U.S. Fish Commission steamer *Albatross*, Alex. Agassiz says: 'I was struck while trawling on our second line between the Galapagos and Acapulco to observe the great distance from shore to which true terrigenous deposits were carried. There was not a station there occupied of which the bottom could be characterised as strictly

[1] See 'Conditions of Sedimentary Deposition,' by Bailey Willis, *Journal of Geology*, vol. i. No. 5, p. 498.

[2] *Bulletin of the Museum of Comparative Zoology at Harvard College*, vol. xxiii. 1892.

oceanic. . . . . A very fine mud was the characteristic bottom we brought up, often very sticky, and enough of it usually remained on the trawl, even when coming up from depths of over 2,000 fathoms, materially to interfere with the assorting of the specimens contained in our trawls.' Logs, branches of trees, twigs, and decayed vegetable matter usually came up with the mud. The distance of the Galapagos Islands from the nearest land in South America is between 500 and 600 geographical miles, while the line between Acapulco and the Galapagos is about 1,100 miles.

The 2,000-fathom line often comes within 100 miles of the coast. Doubtless opposite to the mouths of great rivers, such as the Amazons and the Congo, terrigenous deposits will have a wide extension over the ocean floor. The Indus and Ganges spread their deposits over 700,000 and 900,000 square miles respectively.[1]

There is, however, no doubt that the matters in solution have a much wider extension, only limited, in fact, by the area of the ocean itself. These matters are removed from the water mostly by organic agencies, and consist, in the largest proportion, of carbonates and sulphates of lime. Whether any direct precipitation of the matters in solution takes place in the ocean bottom is a subject of surmise, but there are grounds for believing that they may be chemically deposited as well as organically separated.

[1] 'Conditions of Sedimentary Deposition,' p. 500.

Orographic Relation between Mountain Ranges and New Land Areas.—Leaving for future consideration the regional variations of level which occur in the earth's crust like slow pulsations, and which do not appear to be directly connected with sedimentation, we will briefly examine the evidence pointing to the relation between mountain ranges and new land areas. It is a pretty well established fact, due to the labours of Hall, Le Conte, Dana, and numerous other investigators, that mountain ranges are built up out of great thicknesses of sediment. Upon this phenomenon is based my theory of the origin of mountain ranges by sedimentary loading and cumulative recurrent expansion.[1] The evidence that mountain ranges are composed of great thicknesses of sedimentary rocks, often with very little unconformity between the rock-groups of which they are built up, is clear to any one who takes the trouble to carefully examine the sections, maps, and descriptions of any of the known mountain areas in any part of the globe. It is true of the Rocky Mountains, the Andes, the Himalayas, the Alps, the ranges of the Caucasus, the mountains of the Turco-Persian frontier; as also of the older chains, such as the Appalachians and Urals, which have been greatly denuded. It will, no doubt, turn out to be equally true of the Thian Shan and the great

[1] *Origin of Mountain Ranges*, London, 1886. Also see 'Outline of Mr. Mellard Reade's *Theory of the Origin of Mountain Ranges*,' *Phil. Mag.*, 1891, pp. 485-96.

ranges of Central Asia bordering Chinese territory; but these have been, so far, very little studied.

An examination of the excellent geological map of the world which the labour of Jules Marcou has given us, and which records all information up to the date of its publication, will show that the rocks comprising the newer mountain chains, which are generally considered to have been upheaved in Tertiary times, have a wide extension beyond the limits of the main mountain masses. Roughly speaking, the Cretaceous and Tertiary may be said to occupy the half of Europe and a large area of Northern Africa, together, doubtless, with the bed of the Mediterranean Sea. It is just where these rock-groups are most developed and underlain by more or less conformable Mesozoic groups of great thickness that the mountain masses occur. This is true even of the Apuan Alps, as shown in Stefani's excellent sections.[1] These rock-groups appear to extend from the Caspian to the Himalayas, but further eastward we get largely into the unknown.

In North America, though we have not such full information as of Europe, the Cretaceous appears to occupy, or did occupy (having been denuded from large areas), about half of the continent; and the same may be said of South America, though in both continents large areas remain to be geologically mapped. It must be understood that

[1] *Le Pieghe delle Alpi Apuane, contribuzione agli studi sull' origine delle Montagne*, Firenze, 1889.

I am merely speaking in the rough, to convey the idea I wish to impress upon those who read this, and subject to future correction.

Though there are freshwater and fluviatile deposits among these rock-groups, especially in the Tertiary, the greater bulk are of marine or estuarine origin. There are also great tabular masses of igneous rocks,[1] especially in North America, and volcanic action has over all the areas named played a prominent part from the Tertiary until recent times.

In addition to this great development of Cretaceous and Tertiary rocks on the land areas, there is, no doubt, a great deal—we cannot say how much—below the sea. The West Indian islands give evidences in their fauna of a former land connection with South America;[2] and it may be that the whole area of the Gulf of Mexico and the Caribbean Sea is occupied by the same deposits, overlain by great thicknesses of post-Tertiary accumulations. There is little doubt that much of the sea-bed of the Behring Sea and the Aleutian Islands, together with a strip of the sea-bed (none can say how wide) along the western coast of North America, was land in Pleistocene times,

[1] See 'Report on the Geology of the High Plateaus of Utah,' 1880, U.S. Geographical and Geological Survey of the Rocky Mountain Region, by Captain C. E. Dutton; also 'A Geological Reconnaissance in Southern Oregon,' by Israel Russell. *Fourth Annual Report of the U.S. Geol. Survey.*

[2] See Letter No. 3 by Alex. Agassiz to C. P. Patterson, 'On the Dredging Operations of the U.S. Steamer *Blake* from 1878 to 1879.' *Bull. Museum of Comparative Zoology, Harvard College*, p. 299.

as proved by the discovery of mammoth remains on Pribilof Islands and the N.W. coast of America,[1] as also on Santa Rosa, one of the largest of the coast islands of California;[2] and it is not improbable that Cretaceous and Tertiary rocks may occupy much of this area also.

Without traversing the whole of the known globe and making incursions into the China seas, Malayan Archipelago, New Guinea, New Caledonia, Australia, and New Zealand, it may be well to mention that Tertiary rocks are found far to the north on the land bordering the Arctic seas.

It is in these Tertiary areas that the greatest mountain chains of the world are situated; and it is further conceded by most geologists that they are the highest, because they are the newest, and have not been exposed to such long-continued denudation as, for instance, the Appalachians and Urals.[3]

Establishment of New Land Areas connected with Mountain Upheaval.—It is evident from the foregoing considerations that the establishment of new land areas, whether as continental additions

[1] Dr. George Dawson, *Q. J. G. S.*, vol. l. pp. 1–9, 1894.

[2] 'The Flora of the Coast Islands of California in relation to Recent Changes of Physical Geology,' J. Le Conte. *Bulletin viii., California Academy of Sciences*, pp. 515–20. Extraordinary fluctuations of level are also recorded in the successive sea cliffs up to 1,200 feet above sea-level in the island of San Clemente, off the coast of Southern California, in Pleistocene times. See 'Post-Pliocene Diastrophism of the Coast of Southern California,' by A. C. Lawson. *Bulletin of the Department of Geology, University of California*, 1893.

[3] 'Contributions to the Study of Volcanoes,' Judd, *Geol. Mag.*, 1875, pp. 143–52. 'The Geological History of some of the Mountain Chains and Groups of Europe,' Ramsay, *Mining Journ.*, 1875.

or otherwise, is closely connected with mountain upheaval : the two go together. As I have shown, mountain ranges are never formed on the sites of old denuded land areas; they never arise in the middle of a continent, but are always preceded by great sedimentation. An old continent may subside in the middle or at the margin, and the inland seas or other submerged areas receiving the sediment may in process of time be upheaved vertically and folded horizontally by lateral pressure, and so become eventually an integral portion of the old land. In my 'Origin of Mountain Ranges' I have sought to show that this movement is due to expansion, the initial cause being the heating of the sediments and undercrust by the rise of the isogeotherms or surfaces of equal temperature brought about by the accumulation of sediment. It is unnecessary to explain this principle here, as it can be studied in the original work; but I am pleased to point out that Mr. W. J. McGee adopts this theory in his 'Memoir on the Pleistocene History of North-East Iowa,'[1] with the suggestive addition, that since the sediments rest on inclined surfaces, and are themselves inclined, the movement of expansion must have taken the line of least resistance, and the expanding strata must have moved seawards.[2] Sedimentation, land-

[1] *Eleventh Annual Report of U.S. Geol. Survey*, 1889 90, pp. 351.

[2] Mr. McGee says: 'The sediments of the successive mantles wrapped about the young continent were dropped from the cold waters by deep ocean, and were long chilled by contact with the waters; yet by the end of the Silurian they were so deeply buried as to no longer

making, and mountain-building, we have every reason to believe, are directly related in the chain of cause and effect.

Stability of Conditions indicated by the Accumulation of Great Thicknesses of Sediment over certain Areas.—The accumulation of great thicknesses of sediments extending over millions of square miles, such as took place in Cretaceous and Tertiary times over what is now the western half of the North American continent, points to an extraordinary constancy of conditions over a vast period of time. Greater than this, perhaps, is the record of the earth's history within the Appalachian chain at an earlier period, ending with the close of the Palæozoic. We will, however, for the present confine ourselves to the first-named area, in which are situated the Rockies, the Cascades and Coast ranges, extending, with their analogues, through sixty degrees of latitude from Central

feel the chill waters, and to be heated by conduction from the earth's interior; and so the isogeotherms, or planes of equal temperature, rose from the former ocean floor into and through the lower sediments, and their temperature was greatly increased. As they were heated they expanded; a part of this expansion was vertical, and so peripheral portions of the continent were elevated, not by the building up of successive sheets of sediment, but by their own expansion; yet a considerable part of the expansion must have been horizontal, and must have resulted in either mass or particle movement, or both combined; and since the sheets of sediment rested on inclined surfaces, and were themselves inclined, the movement of expansion must have taken the line of least resistance, and the expanding strata must have crept seaward, while the strata at some distance from the shore line must have been compressed horizontally, perhaps buckled and crumpled, and thrown into anticlinal and synclinal folds concentric with the shore, or elsewhere crushed and broken into fragments along the planes of least strength.'

America to Alaska. It is almost impossible, through our present lack of information—though the geology of the North American continent has progressed with rapid strides—to sketch out the disposition of land and sea which endured through the Cretaceous and Tertiary.

It is highly probable, however, that the old Laurentian areas of Canada and the eastward areas of the United States, with other land stretching into the Atlantic, provided at least part of the sediment. It is pointed out that the deposition of sediment, leaving out of account matters in solution, takes place over a smaller area than that of the land from which it is derived. This would seem to indicate that though general conditions of stability of land areas obtained, fluctuations of distribution occurred throughout the history of these periods in North America. Land to the westward, an extension of the Asiatic continent eastwards, may very likely have sent important contributions towards the making of the future land. But, as I have said, these are speculations which, if we had all the complicated facts before us instead of buried beneath the sea, might not yield satisfactorily to imaginative reconstruction, much less to minds in the position that ours are now.

There is little doubt that many fluctuations of level occurred, and that sediments were raised above the level of the sea and again redistributed. Still, on the whole, the *loci were* the same, and so

in time the sea-bed got built up, until the internal forces of the earth, reacting upon this varied sedimentary load, began their *rôle* of mountain and land building, and the establishment of the western area of the North American continent.

In like manner we might trace the growth of the European continent as it now exists, together with that of a large part of Asia, where the record of the rocks tells tho tale of long-continued periods of sedimentation, culminating in the upheaval of the Alps, the mountains of the Caucasus, the ranges of Turco-Persia, and the Himalayas.

The story might be extended to South America and Australasia, but for the purpose of illustration the examples given are sufficient.

The later rocks of the series more or less represent land conditions, as they consist of lacustrine and fluviatile deposits, showing that the establishment of these land areas dates from a very early time.

The history of these Cretaceous and Tertiary areas is one of both maturity and decay. They are in some portion of the latter stage now, and eventually will probably give place to new land areas formed out of rocks now being constructed in the sea-beds from their own dissolution.

Lithologic Characteristics and Differences of the Rock-groups of some of the Great Periods.—It is a remarkable fact, but no less true, that there are not only palæontological homologies running through the rocks of the Tertiary group over the

known world, but lithological analogies also. Though conditions are recurrent through the ages, as pointed out so strongly by the late Sir Andrew Ramsay, it is recurrence with a difference. There has been lithological evolution as well as organic.

When we go further back in the world's history, and consider the rocks constituting the Carboniferous formation, the persistence of characteristics over large areas of the earth's surface is most striking. The repetition of coals, sandstones, and shales in the Carboniferous of the North-American continent and in that of Great Britain seems almost to point to a common origin. They repose upon great limestone formations, indicating deep-sea conditions. The coal formations in both countries indicate, according to all reliable observers, a gradual sinking of the earth's crust and simultaneous building up of the land by sediments of sand and mud, so that successive terrestrial growths and land surfaces are marked by each bed of coal; excepting, indeed, in those cases that may arise from floating vegetation gradually getting waterlogged and sinking upon an estuarine floor. These long-prevailing conditions are represented by thousands of feet of rocks. It is evident from the lithological resemblances, the occurrence of minerals in the same form and from similar orders of succession, that the physiographic conditions producing them were of world-wide extent and vast continuance. The character of the flora was the same in the

arctic as in the temperate regions, and doubtless the maintenance of similar geographic conditions was one element in this result.

If, again, we compare the Trias of Great Britain with that of the United States, we find that the same characteristics occur in both. They are not at all like the Carboniferous, which they succeed through the Permian phase; and though the broad Atlantic now divides them, they indicate like physiographical conditions—strange to say, they are even of the same colour, deep red and grey, and the characteristic fossils are footprints of reptiles. Red sandstone, red and grey marls, and beds of salt predominate. Gypsum is a characteristic mineral. There is strong evidence of inland salt lakes, lagoons, and tidal shores.

It is often said, and with great force, that it is impossible to determine the age of a rock or the formation to which it belongs by its lithology. That is true, looking at the rocks separately and individually, for the same individual conditions occur in all the ages. Notwithstanding this, broad lithological resemblances or groupings exist. There are coal beds in the Western States of North America, but no one would mistake the Laramie or Tertiary rocks of those regions for Carboniferous. Nor could he mistake the Oolite rocks in Scotland, in which coal is found, for the Coal measures, still less the Lias, though ironstone occurs in it, and is worked to a large extent in the Cleveland district

of Yorkshire. There may be land and sea conditions and deep-sea conditions represented in the rocks of any age, but, independently of the fossils, there are characteristic rock-groups which distinguish the one period from the other.

Even so superficial a stratum as the Drift has common characteristics over Britain, Europe, Asia, and North America—a fact which does not merely point towards analogous climatic influences, but towards similar physiographic conditions on a large scale. Indeed, some geologists think the Glacial period resulted from the physiographic conditions which then obtained. Then, again, to come towards historic time, the post-Glacial deposits that fringe our coasts, the estuarine clays and forest-beds, have a striking resemblance over very large areas, and even continents. Indeed, so important seemed these characteristic similarities and differences to our predecessors in the science of geology, that they constructed, wiped out, and reconstructed, the earth's surface and crust again and again by the aid of cataclysmic convulsions. The influence of Lyell was all towards showing the fallacy of these inferences; but later the pendulum swung too much the other way, and we were led to dwell inordinately on continuity and recurrence.

The uniformitarian theory was of great value in bringing men's minds down from the region of pure imagination to that of fact. It was a good working hypothesis, but, like most generalisations,

was a little one-sided. It represented too exclusively one side of the phenomena of the earth's history, to the neglect of another—that of development. I am not one of those who think the doctrine of uniformity a 'fetish,' but rather a theory that kept us within the range of practical fact. For my part, I am prepared to accept anything that can be *proved*, but until the proof is forthcoming we are on safe ground if we confine ourselves to agencies of which we have experience.

Persistency of Drainage Lines

The ideas and considerations dealt with in the preceding part of this chapter bring us naturally to what is taking place in our own time. Since the Tertiary upheavals new lines of drainage and channels of erosion have been established, by which the waste of the land has been and is conveyed to the lowest levels, which are generally, but not always, in the ocean. That is to say, in some cases the drainage lines terminate in inland seas or hydrographic basins, in which the evaporation exceeds the rainfall. Such are the Aralo-Caspian basin, the Central Asian basin[1]—in which are situated Lakes Balkash and Alakul—and the Great Salt Lake and other areas in North America. Mediterranean seas, connected by an outflow or inflow, or both, like the Black Sea and the

[1] See 'The Physical Conditions of the Aralo-Caspian Region,' Wm. Hewitt, Pres. Address, Liverpool Geol. Soc., *Proceedings*, 1892 3.

Mediterranean, and bays such as the Gulf of Mexico, the Gulf of Californa, Hudson's Bay, &c., also receive their quota of sediment.

Though the general levels of the continental lands have fluctuated, the main drainage lines—established and deeply cut into the strata they traverse—seem to be very persistent, so that the waste of a large part of the North American continent has travelled down the Mississippi to the sea since Tertiary times. Similarly, that of the South American continent has followed the lines of the Amazons, La Plata, and Orinoco; while that of the western area of both continents, cut off by the divide of the Andes and Rockies, extending from south lat. 50° to north lat. 70°, has perforce been cast into the Pacific Ocean or the embayments connected with it.

In the continent of Africa the Congo delivers its load into the broad Atlantic, nearly opposite to the Amazons, with a flood almost equal, if not superior, to it. The same persistence is seen in the rivers of Europe and Asiatic Russia and the northern portion of North America delivering into the Arctic Ocean, and in the great rivers of China flowing eastward into the shallower eastern seas. These lines of drainage, as I have sought to show in the 'Origin of Mountain Ranges,' have become fixed in their positions through the upheavals of mountain ranges; and the modifications they have undergone in their courses have been mainly confined to the hydrographic basins in which they

exist, the most fluctuating portion of the rivers being on their own deltas. In cases where the levels of the watershed may not have been very marked, alterations of the drainage basins themselves have arisen through differential subsidence or elevation. Changes such as have been noted in the Oxus and Jaxartes, in Central Asia, have hardly involved a rearrangement of drainage basins considered on the large scale. The Ganges and Brahmapootra have rolled their floods into the Bay of Bengal since the final upheaval of the Himalayas. But it is unnecessary to multiply these instances.[1]

A Future Period.—It is now time to ask ourselves what is the meaning of this constancy and the persistence of these important physical features. Though differential subsidence and elevation on the large scale are marked on the one hand by raised beaches on the land, and on the other by deep-cut river beds below the level of the sea, as in the Congo and the great rivers on the North American continent, what may be called the rugosities of surface represented by mountains never disappear excepting by denudation. Further rugosities get developed by faulting and mountain elevation in the *locus* of mountain ranges, by a persistence of the movements which have initiated them; but the great levellers, the

[1] Ramsay's 'River Courses of England and Wales,' *Q. J. G. S.*, 1872, and 'On the Physical History of the Rhine,' *Royal Inst.*, 1874, may be studied with advantage in this connection.

waters and the atmosphere, with their combined chemical and dynamic action, bring their materials again under the displacing action of gravity, whereby, like water itself, they finally find their lowest level. Hence sediments accumulate and have been accumulating through Quaternary time, and were these actions to continue long enough without compensatory elevation, the whole of the land, as has been pointed out again and again, would disappear. On the other hand, were elevation to go on long enough, the whole of the sedimentary deposits would be stripped from the land, and we should be permitted to see what no one is certain he has ever yet seen—that is, the original crust of the earth.

It thus appears that, though these denudations are of long continuance, they must in the course of geologic history come to an end, for we find most of the land areas covered with sedimentary deposits.

It follows, then, if the geologic history of our planet is to continue in the manner in which it has done in the past, and not to terminate in stripped land areas or universal levelling, that the sediments on the coasts and seas bounding our continents, which have been accumulating since Tertiary upheavals, must themselves be eventually raised above the waters and joined on to the dry land. In what way has this occurred in the past? *If we ascertain correctly, it will be the key to the future*—a truly geological prophecy. That there is a relation of

cause and effect between subsidence, sedimentation, and subsequent upheaval, it has been my object to point out in the 'Origin of Mountain Ranges.' In development of this idea I seek to show how, as a consequence of this action and interaction, the earth's strata come to be arranged in rock-groups containing distinguishing fossils and having each its characteristic lithological grouping, which we classify as periods.

If a great group of physical features lasts throughout vast eons of time, such as I have shown has happened even during the Quaternary period (which we are living in now, and which is likely to continue much longer), it is evident that the deposits will have a characteristic lithology—looked at in the large way—and a fossiliferous *facies* will distinguish them from the Tertiary and preceding rock-groups. Representatives of the modern mollusca and the vertebrate and mammalian fauna, and of the modern flora, will be here and there embalmed to give further distinction to the strata and joy to future geologists. Among these fossils, works of art and fragments of ships, together with bricks and clinkers from ocean steamers, such as even now are occasionally dredged up in the Atlantic, will be a feature. I understand that when soundings come up with cinders attached to them the heart of the mariner rejoices, knowing that he is on beaten tracks, if so inappropriate a phase is allowable in ocean navigation. Truly, if the world continue through

another geologic period—and there seems no reason to assume that it will not, as the internal forces are still alive, making for rejuvenescence—the Quaternary will be the most distinct and remarkable period of all. Not only will the remains of man and his works up to the present be embedded therein, but also other works still in the potentialities of the future, to which we may, considering the progress of the last fifty years, look forward with mysterious expectation, if not awe. The future great period will include the present and terminate with the completion of the Quaternary.

Sediments of Existing Seas.—If the preceding reasoning has any cogency in it, there must exist on the coasts, at the mouths of the great continental rivers, enormous sedimentary deposits laid down since the close of the Tertiary. The denudation of the land since then, though late in geologic time, has been enormous. Whole areas have been stripped of their Tertiary covering, and in mountain districts thousands of feet of strata removed. To go no further than our own isles, the Tertiary rocks that remain are but a fragment of what once existed.

The Miocene rocks of Antrim, Staffa, Eigg, Rum, and Skye consist chiefly of lava flows and ashes of great terrestrial volcanoes, which fill up the undulating valleys of the Chalk in Antrim, and those of Oolite and Silurian gneiss in the West of Scotland. According to Ramsay, the Western islands formed part of a 'vast continent, to which

the British Islands were united, and which, embracing Iceland, spread far to the north and west into the area of what is now the Atlantic, and on the south was united to Africa, when as yet the Mediterranean had no existence.'[1]

Long before the extreme denudations represented by these fragments of a once continuous sheet took place, old rivers intersected this ancient land and scooped out valleys in the Miocene lavas and hills, which were again partly filled by torrents of basalt and obsidian. 'Thus it happens that in the old volcanic plateaux, valleys a thousand feet deep have been excavated, and the whole region has by denudation been changed into a line of fragmentary islands, the high sea-cliffs of which attest the greatness of the waste they have in time undergone.'[2] Sir A. Geikie says, speaking of Eigg: 'Lastly, from the geology of this interesting island we learn, what can be nowhere in Britain more eloquently impressed upon us, that, geologically recent as that portion of the Tertiary period may be during which the volcanic rocks of Eigg were produced, it is yet separated from our own day by an interval sufficient for the removal of mountains, the obliteration of valleys, and the excavation of new valleys and glens where the hills then stood.'[3] Though we may not claim all this denudation for Quaternary time,

[1] *Physical Geology and Geography of Great Britain*, 5th ed., p. 263.

[2] *Ibid.* p. 356.

[3] 'On Tertiary Volcanic Rocks,' *Q. J. G. S.*, vol. xxvii. p. 310.

since much of it may have taken place during the Pliocene, with resultant Pliocene rocks now sunk beneath the sea, these quotations from eminent geologists may well serve to give us an inkling of the powers we are dealing with. According to Dana, the length of the Quaternary period up to now is one-third of the Tertiary.[1] This is but an approximate guess, but nevertheless a valuable one.

Turning our attention to North America, we may say that the Mississippi is at least as old as the Quaternary, and probably very much older. The elevation of the Rocky Mountain regions compelled the drainage of the continent to take a south-westerly course, while the older land of the Laurentian highlands and the Appalachians blocked it from direct connection with the Atlantic, except in the northern portions. It has been shown that since the elevation of the Uinta Mountains, which began in Cretaceous times, three and a half cubic miles of rock have been removed from every square mile of their surface.[2] A large area of the central Mississippi Valley is occupied with Cretaceous rocks, while the southern part, bordering the Gulf of Mexico, is Tertiary (Eocene and Miocene), and a smaller

[1] *Manual of Geology*, 2nd ed., p. 586.

[2] *Origin of Mountain Ranges*, p. 243. In a most interesting paper by Dr. Andrew Lawson, entitled, 'The Post-Pliocene Diastrophism of the Coast of Southern California' (*Bulletin of Dept. of Geol. University of California*), it is shown that Pliocene sediments of over a mile thick, called the Merced series, were laid down on the Californian coast. This is an extraordinary example of the accumulation that has taken place in only the closing phases of the Tertiary period.

portion consists of Quaternary deposits. This would seem to show that for a considerable period of time after the upheaval of the Rocky Mountain region continental conditions prevailed over this area, and that the sediments from its erosion and waste now lie at the bottom of the Atlantic.

Unfortunately, we cannot read the earth's history with any fulness or accuracy. We may only see as through a glass darkly; but still we are justified in concluding from a consideration of these examples, which could be multiplied from wellnigh all the known world, that an enormous erosion of the land has taken place during the Quaternary period, and that these sediments, probably with those of an earlier period, now lie on the ocean floor, are still accumulating, and will continue to accumulate until such time as the earth's living forces bring them up from below the waters, to take their place as mountain, plain, and valley in the unceasing cycle of the earth's changes.

Conditions of the Earth's Crust in which Sediments accumulate, and Rate of Accumulation.—The conditions of the crust of the earth upon which these sediments are being laid down are as varied as the sediments themselves. While in the northeastern portions of North America, from New York to Baffin's Bay, there are no evidences of volcanic activity, either in the present time or late geological past, south of New York we gradually approach a volcanic and earthquake area, crossing the Gulf of Mexico, and culminating in Central America.

The West India islands give frequent evidence of volcanic instability by the raised coral formations which are there met with, together with foraminiferal deposits considered to be of deep-sea origin.[1] Within this basin-shaped and almost closed Gulf of Mexico deep-sea oozes are being laid down, and into this area, at one locality or another, the Mississippi has delivered its daily burden of sediment through at least Quaternary time. These spoils of the continental land will probably be interbedded with the lime deposits worn from coral reefs, and with the more purely oceanic accumulations of foraminifera and other deep-sea forms of life.[2]

Fluctuations of level of the Mississippi mouth and valley may have given the terrigenous deposits a wider distribution than what obtains now. Borings in the Mississippi Valley show a subsidence of at least 630 feet at New Orleans.

If the land were upraised so as to represent the physiography of the time of this elevation, the sediments now being brought down this great drainage channel would be delivered further into the Mexican Gulf. Likely enough, in portions of the Gulf in time past there may have been outflows of lava on the sea-bed. Volcanic

[1] 'On the Elevated Coral Reefs of Cuba,' W. O. Crosby, *Proc. Boston Soc. of Nat. Hist.*, vol. xxii.; 'The Geology of Barbados,' Jukes-Brown and Harrison, *Q. J. G. S.*, 1892, pp. 170–226.

[2] See three letters by Alex. Agassiz 'On the Dredging of the U.S. Steamer *Blake*,' *Bull. Museum of Comparative Zoology, Harvard*, vol. v. 1878.

intrusions in the form of sheets insinuated between sedimentary beds are another form in which it is highly probable these igneous forces have developed themselves. If there be any truth in these inductions, there must be immense accumulations existing in the Gulf of Mexico, for its area is not above one-third of that from which the sediments have been derived.[1] In addition, we have the great denudations from the mountainous regions of tropical America and from Mexico.

The land sediments and the lime and silica eliminated from the waters of the sea by organic agencies, and ever being renewed by the decomposition of the rocks through the solvent action of rain-water, aided by humic acids, must, together with igneous flows, intrusions, and ashes, be building up rock-groups which will eventually form the framework of new land. None the less is this the case when the earth's crust has been long in apparent repose; for there surely will come a critical time, when they will begin to react on the earth's interior, so that before being elevated into new land these portions of the earth may go through the volcanic cycle of change. We might in this way travel over the earth's surface, and find in every part modified phases of the same actions in

[1] In a valuable paper, entitled 'The Gulf of Mexico as a Measure of Isostacy' (*Ann. Journ. of Science*, vol. xliv. 1892, p. 188), McGee estimates the area of degradation at 1,800,000 square miles, and that of deposition at 100,000 square miles, or one-eighteenth. This may be true in relation to what is now taking place, but my assumption is that the area of deposition has shifted from one *locus* to another, being conditioned by elevation and subsidence.

progress—for our planet has not yet lost its vitality. All along the west coast of the two Americas volcanic activity is adding to the thickness of the sea-bottom, and the ceaseless denudation of the great mountain ranges provides a covering on a still grander scale. On the eastern coast of South America the conditions of deposition are less variable, but a still greater load of sediment is being deposited in the Atlantic by the Amazons, La Plata, Orinoco, and other great rivers. On the African coast it is the same; and two of the greatest rivers of the world—the Amazons and Congo—pour their tropical floods and the spoils of the land on opposite sides of the Atlantic in the same parallels of latitude.

I have shown that the sediment from 21,000,000 square miles of land, the estimated area of the land draining into the Atlantic, would, assuming the rate of denudation to be 1 foot in 3,000 years, fill up an equivalent area in the North and South Atlantic, estimated at two miles deep in 32,000,000 years.[1]

With all this variety I have little doubt that the deposits as a whole will be differentiated from any that have gone before, so as to justly enable the time in which they were laid down to be called a 'period.'

This will be due mainly to the persistence of the physiographic features of the land areas, which

[1] 'Denudation of the Two Americas,' Presidential Address, Liverpool Geol. Soc., 1885; see reprint in Book III.

will only change when the internal forces of the earth, reacting on the sedimentary mantle, develop expansion therein, and by lateral and vertical pressures and movements, oftentimes renewed, develop those ridges of the earth called mountain chains, and so diversify the planet's surface by sketching out new land surfaces where now is sea, thus modifying the form and conditions of the old continents.

General Conclusions

The result of our reflections upon the group of facts which it has been my object to bring together in systematic order tends to show that the growth of land areas of the globe is governed by certain laws of development. The records of the rocks tell us pretty plainly that there must, throughout geologic time, have existed land areas on the globe comparable in extent with those now existing. It is not my intention in this chapter to touch upon that vexed question, the permanence of ocean basins, or to sketch out the lines of former land extension. We have seen that land areas grow by accretion from existing land. The ruins of former continents have added to their extent, so that by process of accretion their outlines and physiographic features have altered; therefore the present continents, though the outgrowth of earlier ones, may be vastly different in form, position, and orography from their predecessors. That the land areas should have been preserved through geologic time,

considering that their mean heights are so little above the water, has always presented itself to my mind as a geological crux. We now see that the waste of the land and the collection of the resultant sediments in the bordering seas are nature's means of renewal; and we further gather that continuity of land areas throughout geologic time, so necessary for the preservation of terrestrial life, is in this way secured. The origin of mountain ranges and the growth and decay of continents are thus closely related. New lands are the consequents of sedimentary loading and recurrent expansion, acting through a chain of events which I have dealt with in the 'Origin of Mountain Ranges,' and which seem to me to be the explanation which brings together all the hitherto isolated facts of geology into one comprehensible whole. The history of our planet is not one of fortuitous accident, but of orderly development, the principles of which it is the aim of this work to investigate and establish.

CHAPTER · IV

OCEANOGRAPHY

SUB-OCEANIC CONFIGURATION OF THE EARTH'S CRUST

INTRODUCTION.—The evolution of the geographical forms of the land areas of the globe having been considered, it will be necessary in relation thereto to further study the ocean basins and the complex questions included under the term 'oceanography.'

It is only within the last five-and-twenty years that anything of an accurate nature has been known of the form of the ocean bottom. Surveys and soundings there had been, but these were mostly in shallow depths and near to land.

The *Challenger* expedition enlarged our knowledge on a systematic scale; but these soundings in the deep seas were necessarily taken at such distances apart that, except as giving a rough idea of the average depth of the ocean on the tracks that the ship followed, little could be inferred.[1]

With the extension of commerce and the laying of deep-sea cables came the necessity for more

[1] It is necessary to acknowledge the work of our Navy of late years in Pacific deep-sea soundings, as well as the surveys of the U.S. Fish Commission in the Atlantic and the Steamer *Blake* in the Gulf of Mexico and the Pacific.

accurate knowledge of the form of the ocean bottom; but even now it is only in exceptional cases that anything like a complete set of soundings, sufficient to enable a section to be plotted, is taken. The result of these more complete surveys is, generally, to show that the contours of the ocean bottoms are much more diversified than was formerly inferred. I have pointed out in a former paper[1] that if the present continents were submerged to the depth of some of the great seas, a series of soundings taken as far apart as those of the *Challenger* would show no more variety in depth than do the *Challenger's*, and that the inference that the ocean basins are little more than vast plains or saucer-shaped depressions, with here and there volcanic cones protruding above the surface of the waters or stopping short at various depths below, was not based on sufficient data.

The Sub-Oceanic Margin of Spain and West Africa.—Soundings from the Bay of Biscay along the West Coast of Africa reveal greatly diversified submerged ground, which appears to have a considerable effect in inducing deep-sea currents.

In relation to these soundings and the nature of the sea-bottom deposits, Mr. Stallibrass, in a paper on 'Deep-sea Soundings in connection with Submarine Telegraphy,' says, 'Of all bottoms these oozes are to be preferred. The fact of their being found shows that no currents exist in these parts, and

[1] 'Oceans and Continents,' *Geo. Mag.*, vol. vii. 1880, p. 388.

they are so soft that the cable sinks far down into them. The old idea that currents do not exist at any great depth has long since been rejected. Currents may exist at almost any depth. Between the Canary Isles there are strong currents 1,000 fathoms below the sea face, and their scouring action may be clearly detected.'[1] Mr. J. Y. Buchanan, in the discussion of the paper above referred to, expressed similar views, and added, 'If we find hard ground we know that there must be something to prevent the accumulation of sediment. Now, the only thing that prevents the accumulation of sediment is a current; and one help that telegraph soundings have given to geographical science is the indication that tidal currents exist even at very great depths in the open ocean.' Mr. W. H. Huddleston, in a paper on 'The Eastern Margin of the North Atlantic Basin,'[2] gives a considerable amount of information as to the form of the ocean floor and the gradients of what he terms the sub-oceanic slopes, which, as he shows, are exceptionally steep in the Bay of Biscay off the coast of Spain. 'Off Rivadeo the 100-fathoms platform is of the usual width for this coast—nearly 30 miles, in fact; from the edge of the platform a depth of 1,130 fathoms is obtained in the next 5 miles, giving an incline of about 1 in 4, or about 30°.'[3] About 30 miles to the north of Bilbao is a channel from 1,000 to 1,500 fathoms

[1] *Journ. of the Soc. of Telegraph Engineers*, p. 509 (1887).
[2] *Geo. Mag.*, March and April 1899. [3] P. 153.

deep, apparently sunk in a submerged platform which itself has an extremely irregular surface. This is a place notorious for cable fractures, which usually happen in the month of March, when sometimes portions of the cable four and five miles in length are buried. 'The fractures are attributed to the action of a submarine current, caused by the piling up of the surface water cutting the prolongation of a river-bed with steep walls; these walls when undercut are believed to fall in such masses as to bury the cable. No rock is marked on these soundings, which yield clay, stiff clay, mud, and sand. In this locality the 100-fathoms line lies 12 miles off the coast.'[1]

Professor Hull, who has paid considerable attention to the submarine configuration of the coasts of Western Europe and the West African continent,[2] considers that the 'Great Declivity' or seaward fall of the bottom from the 100-fathoms line represents along its whole length in Europe and Africa a submerged sub-aërial escarpment. He gives much information as to the sub-oceanic continuation of the river beds across the 'Continental Platform' and the 'Great Declivity,' giving the best evidence of the strength of his convictions by charting their several courses. That there have been fluctuations of level between the sea and the land in times geologically recent, to the vertical

[1] P. 152.

[2] See volumes xxxi. and xxxii. of the *Transactions of the Victoria Institute*.

extent of 1,200 fathoms, must be conceded, but whether they were simultaneous on both sides of the Atlantic, as he appears to maintain, is not so certain. Such an enormous change would mean a sinking of the ocean bottom to a relative cubical extent elsewhere, otherwise the waters would rise with the land and conceal the real vertical movement.

Though Hull is an extremist in these views, there is a large substratum of truth on which they are founded.

Varied Configuration of the North Atlantic Bottom.—In a presidential address to the Liverpool Geological Society, in 1885, on the North Atlantic as a Geological Basin (reprinted for reference in Book III.) I expressed the opinion that as soundings were multiplied the more uneven the sea bottoms would prove to be, and the general correctness of this opinion has been verified since.

It so happens that the views put forward in the address referred to have been amply tested and strikingly confirmed in the North Atlantic by the sounding expedition organised by the Deutsch-Atlantische Telegraphengesellschaft and the Commercial Cable Company, which ran two lines of soundings between the British Islands, the Azores, and North America. It was also ascertained that the contours of the submerged portion of the Azores were of so diversified a nature that it was really impossible to follow them out in anything like detail. In this respect the experience seems to

have been the same as that recorded by Sir James Anderson between Lisbon and the Canary Islands.[1]

The connecting lines between the Azores and America on the one hand, and England and Ireland on the other, prove that the ocean bottom between the continents of America and Europe is anything but the extended plain it was thought to be.

'The two lines of soundings run between the Azores and the British Islands are very interesting, for they both developed uneven ground. On the more northerly line the bottom gradually sinks from the 1,000-fathoms line off the south-west point of Ireland down to a maximum depth of 2,693 fathoms in lat. 45° 49′ 18″ N., long. 19° 39′ 26″ W.; then shoals slightly to 2,145 fathoms, and continues to rise and fall in a series of undulations more or less abrupt until the shallow water of the Azores bank is reached. The 2,145 fathoms sounding is succeeded by one in 1,667 fathoms, and then, in lat. 43° 59′ 11″ N., long. 22° 7′ W., by one in 1,410 fathoms.'

It is unnecessary to quote further as to this line of soundings, except to say that two submarine elevations were discovered, represented by soundings of 1,410 and 1,200 fathoms respectively.

The more southerly line between the Azores bank and North America 'is extremely interesting, and may be dealt with in greater detail.

[1] See *The North Atlantic as a Geological Basin:* Reprint in Book III.

'Off the American coast the soundings deepen gradually down to 2,300 fathoms; then in latitude 39° 45′ 30″ N., long. 66° 16′ W., a cast was taken in 1,675 fathoms, apparently surrounded on all sides by water several hundred fathoms deeper.

'From this point the bottom sinks gradually, and in lat. 40° N., long. 63° W., three deep soundings exceeding 3,000 fathoms were taken in what has been called the Libbey deep, the depths being 3,144, 3,237 and 3,318 fathoms, the last-mentioned being the deepest cast taken during the cruise. Proceeding eastwards from the Libbey deep, the water shoals gradually to 2,768 fathoms, but in lat. 40° 11′ 51″ N., long. 59° 51′ 30″ W., a single deep cast in 3,045 fathoms was taken, followed by a sounding in 2,810 fathoms, and then another deep cast in 3,160 fathoms in lat. 40° 25′ 30″ N., long. 58° 52′ W. (See Section C to A, Plate II.)

'It thus appears that these two deep casts form two isolated "deeps" separated by shallower water, and the name Sigsbee deep has been given to the 3,045 fathoms depression, and the name Thoulet deep to the 3,160 fathoms depression.'[1]

Sub-Oceanic Contours of the Azores.—In the presidential address already referred to the following statement is made: 'The Azores are situated upon a central area connecting the north coast of South America with Iceland and enclosed by the

[1] *On the Results of a Deep-sea Sounding Expedition in the North Atlantic during the Summer of* 1899 (pp. 3-5), by R. E. Peake, M. Inst. C.E., with notes by Sir John Murray, C.B., &c.

2,000-fathom contour. It is of this area that we want much fuller information.' [1]

The soundings of 1899 fortunately supply us with this. 'The bottom around these islands is now known to be so irregular that none of the usual bathymetrical contour lines can be made use of as a limit to the bank. Thus the 1,000-fathoms line cuts up the group of islands into two separate banks, the one including the islands of Flores, Corvo, Fayal, Pico, San Jorge, Graciosa, and Terceira; the other including San Miguel, Santa Maria, and Formigas.

'The 1,500-fathoms contour includes all the islands of the group, but it would extend the bank far to the north, for in this part of the North Atlantic there is apparently an extensive ridge running north from the Azores, on which several submarine elevations are situated.' A glance at the map accompanying Mr. Peake's description 'shows how uneven the ground is around these islands, with isolated basins and elevations, depths of over 1,000 fathoms being found sometimes comparatively close to the islands.' Then follows this note: 'Further evidence afterwards obtained from the varying strains which had to be carried while laying the cables across the Azores bank tends to show that this area is much more uneven than would appear from the comparatively small number of soundings that the time limit allowed of being taken during the *Britannia* expedition.'

[1] See Reprint, Book III. p. 285.

About 4° north of the Azores there are soundings of only 48 and 70 fathoms; while further north, in lat. 50°, there are three soundings, 625, 730, and 982 fathoms, taken by the Telegraph Company's s.s. *Faraday* in 1882.

A comparison of this chart with the contoured chart of the North Atlantic by Sir James Anderson, published in the 'Origin of Mountain Ranges,'[1] will show how much has been added since 1886.

I would point out that this uneven bottom is not confined to the vicinity of the Azores, but has been proved more or less along the whole of the lines of zigzag soundings taken, but only partly described in the preceding pages.[2]

Not only do the soundings in the North Atlantic tell us that the ocean floor is varied in configuration, but at the Antipodes we meet with facts of the same nature.

Ocean Bottom off Moreton Bay, Queensland.—In an interesting paper by the Rev. W. B. Clarke, F.R.S., 'On the Deep Oceanic Depression off Moreton Bay, Queensland,'[3] we are informed that the soundings of the *Tuscarora* show a depth of 562 fathoms (brown mud and sand) 36 miles from

[1] P. 313.

[2] In a communication to *Nature*, dated from the *Princesse Alice*, with the Prince of Monaco on board, J. Y. Buchanan describes soundings made between Gibraltar and the Azores. The depth of water on Gorringe or Gettysburg bank was found to be very uneven and the surface of the bottom very rough. In contrast with this the soundings on the Josephine bank 'revealed a uniformity of depth which is astonishing' (*Nature*, August 14, 1902, p. 376).

[3] Read before the Royal Society (of Australia ?), July 20, 1876.

Cape Moreton Lighthouse, which increase to 2,485 fathoms 58 miles from the same point, and the line of soundings continued to Kandara Island, 19° 11′ S. lat., 177° 41′ E. long., show continual variations of level, ranging between 645 fathoms and 2,682 fathoms.

This depression is the more remarkable as the immediate coast of Queensland is comparatively low. Mr. Clarke remarks: 'If for the sake of illustration we could raise New Holland, New Zealand, and New Guinea to one uniform additional height of some 2,600 fathoms above the ocean, we would, I think, perceive similar features on the surface so formed to those which are now exposed.' [1]

The Indian Ocean.—The laying of the Cape-Australian cable by the Eastern Extension, Australasia, and China Telegraph Company has put us in possession of a line of soundings of some 7,600 nautical miles. Starting from Natal, South Africa, it passes the southern end of Madagascar to

[1] The deep boring at Funafuti atoll in the Pacific, reaching a depth of over 1,100 feet, I am informed on good authority, began in organic sand and soft and cavernous limestone. The sand gradually disappeared, and the core became compact and stony except for small holes where corals had disappeared. The organic character of the rock remains much the same throughout, though in places particular groups predominate. The main constituents are calcareous algæ, such as *Halimeda*, Foraminifera of various kinds, and corals, mostly of reef-building genera, there being nothing to suggest that the lower part was formed in materially deeper water than the upper.

Though we must await the report of the Royal Society experts before coming to a final conclusion, the cumulative evidence detailed in this chapter points strongly to a very extensive subsidence of the bed of the Pacific, which seems to be reinforced by the information revealed by this interesting boring.

the Mauritius, from thence to the Cocos or Keeling Islands, from which it is continued in a south-easterly direction to Perth in Western Australia. Following the coast, the line of soundings bends round the south-western promontory of Australia, from whence it is continued in a parallel of latitude easterly to the coast of Adelaide.

These soundings, taken together with others shown on the Admiralty Chart of the Indian Ocean, reveal a considerable variation of depth and irregularity of bottom between Natal and Madagascar. The deepest (2,700 fathoms) is about half-way, but on the line of prolongation of the axis of Madagascar, south of the regular line of soundings, there are two of 280 fathoms each. Opposite Fort Dauphin, much nearer to the coast there are depths of 2,200 and 2,500 fathoms. From thence the bottom is fairly regular until Réunion or Bourbon Island is reached.

Here the irregularities again commence, and continue as far as Rodrigue Island. The sea increases in depth from 15 fathoms to 2,945 fathoms about long. 76°.

Eastward of this there is not much variation in the depths until an area between long. 86° and 89° is reached, when the soundings vary from 1,235 to 2,940 fathoms. From this irregular submerged area to Keeling Islands, the soundings are deep, reaching a maximum of 3,310 fathoms.[1]

[1] Darwin describes these atolls in his *Coral Reefs*, p. 240, second edition, 1874.

These islands seem to rise out of a sea 2,500 to 2,700 fathoms deep.

Between Keeling Islands and Perth, Australia, there are depths of over 3,000 fathoms, and some of these run in pretty close to the Australian coast. There are also two considerable submerged areas about halfway between Keeling Islands and Perth, in which the soundings are not more than from 1,500 to 1,600 fathoms.

Australia: West and South Coasts.—The phenomenon of the continental shelf reappears again in Australia, and the soundings indicate sudden falls from this regular platform to the ocean deeps.

The forms of the continental margins already fully described in America repeat themselves in Australia, with variations due to the difference of geographic and geologic structures. It is difficult to resist the conviction that this almost universal feature of a sub-aqueous terrace surrounding continental land is due mainly to deposition of sediment.

The line of soundings off the southern coast shows depths of 2,000 to over 3,000 fathoms, which shallow to 1,300 as the approaches to Adelaide are neared, this latter depth being no great distance from the 100-fathoms platform.

These are a very excellent continuous set of soundings, throwing much light on deep-sea problems. I venture to predict, however, that the configuration of the bottom, when surveyed with the

minuteness of the survey of the North Atlantic already described, will reveal features of an equally varying and interesting character.

It is worth mentioning that at the meeting of the Eastern Extension Company on November 13, 1901, the chairman, Sir John Wolfe Barry, stated that one of their great troubles with the cables was 'fish-bites': 'In this particular half-year we have found the tooth of a shark in a cable at the very great depth of 330 fathoms,' the sheathing of thick iron wires and outer coverings being bitten through.

To describe in further detail what has been ascertained of the form of the ocean floor would occupy too much space. For the present purpose the information given leads to the conviction that some of its features approximate to those distinguishing dry land. Extended plains no doubt exist, but varied with large undulating tracts. Valleys and mountain ranges also are present. The information given in this Chapter certainly lends no support to the view that the ocean bottom is a plain, varied only by submarine volcanic cones rising towards or above the sea-level.

CHAPTER V

PRODUCTS OF DENUDATION ACCUMULATE IN OCEAN BASINS

IN considering this question we must not lose sight of the fact that the seas and oceans are the recipients of the spoils of the land—that hour by hour, day by day, century by century, the products of the denudation of the land are being carried forward and distributed over the sea-bottom. I have shown in 'The Denudation of the Two Americas'[1] how great the amount of this matter in mechanical suspension and in solution is, and have shown by way of illustration that, if we take the area of the South Atlantic from the equator to the 40th parallel—an area in round figures of 10 million square miles—and add to it the area of the North Atlantic from the equator to the 40th parallel, which is in round numbers eleven million square miles, we get a combined area of twenty-one million square miles, approximately equal to the total land area draining into the North and South Atlantic. Estimating the mean depth of this section of the ocean at 2 miles—a very liberal estimate—and the detritus

[1] Republished in Book III.

worn from the land at 1 foot in 3,000 years, in 32 million years the whole area of 21 million square miles would be levelled up. That is, 2 miles in thickness of rock would be removed from the land, and the same thickness of deposits laid down in the ocean over equal areas, always supposing the rock and the deposits to be of the same specific gravity.

I doubt if it be sufficiently appreciated by geologists how important it is to get a firm grasp of the quantitative relations of matter on the globe. Such a knowledge, I find, tends to dissipate many theories that otherwise at first sight may seem very attractive. It is here that the value of geologic time as an interpreter of phenomena is of such value.

Let us now consider how we can apply the lessons of this calculation in assisting us to appreciate the nature and form of the sea bottom. As already stated, the coarser material carried in suspension by rivers is laid down in smaller areas than those from which it has been derived; consequently the deposits increase in thickness at a proportionately quicker rate. Not only is this so, but as the deposit at first is largely saturated with water, the accumulation is still more rapid.

Mr. J. Y. Buchanan says: 'The African rivers are quite stupendous, and have much to do in giving the Gulf of Guinea its peculiar character. The drainage of quite 90 per cent. of the whole

continent empties itself into a very restricted area of the sea, the formation and the conditions of which it has profoundly modified.'[1]

Again, in the report of the Eastern Telegraph Company, January 25, 1900, the following statement is made: 'All cables are subject to temporary disablement from various causes, such as corrosion, submarine earthquakes, &c., and the action of great rivers discharging immense volumes of earth-laden water into the ocean, causing submarine landslips; this is especially the case off the Congo River.'

TERRIGENOUS DEPOSITS CONCEAL SUBMERGED SURFACE FEATURES OF THE EARTH

It is evident from these facts and considerations that within the areas surrounding the continents upon which terrigenous[2] deposits are laid down deposition will quickly modify the features of the bottom, fill up the hollows and smooth off the asperities, and it is to deposition we must look for an explanation of the regular gradients that generally connect the continents with the ocean. If we turn our attention to the Pacific coast of America, we find the same concealment of the underlying rocks by a mantle of recent deposits.

The dredgings of the U.S. Fish Commission steamer *Albatross*, Alex. Agassiz says, proved that the ocean floor between Galapagos Islands

[1] *Nature*, March 25, 1886, p. 495.

[2] In one sense all deposits are terrigenous.

and South America is covered with decayed vegetation and silty deposits from the land at depths as much as 1,100 fathoms. It is 600 miles from Galapagos to Cape San Francisco, in Ecuador. The bottom in places is very uneven. 'When trawling from north to south we seemed to cut across submarine ridges, and it was only when trawling from east to west that we generally maintained a fairly uniform depth.' When in soundings of 1,100 fathoms, 'unfortunately, we deepened our water while towing only twenty minutes to over 1,400 fathoms.' [1]

On all the continental coasts the same phenomena, with variations, occur.

In Lower Burma, the Salween River discharges water laden with $\frac{1}{300}$ part of sediment into the Gulf of Martaban, which is distributed over 2,000 miles, depths of 40 or 50 fathoms being reduced to 15 to 20 fathoms in forty years.[2]

But it is not merely opposite the great rivers that these accumulations occur; they are distributed all along the coast, in varying widths and, I believe, greater volume than is generally suspected.

[1] *Bulletin of the Museum of Comparative Zoology at Harvard College*, vol. xxi. No. 4, pp. 185–200.

[2] *Nature*, Feb. 1901, p. 427.

AGE OF RIVER COURSES AND ITS INFLUENCE ON DEPOSITION.—COAST LINES

It must not be lost sight of that the present river courses date back in most cases to Tertiary times, and though the continents have been at one time partially submerged, and at another elevated above their present level—and this slow pulsation may have occurred many times since the present valleys were excavated—the discharge of sediment has been taking place through these channels all the while. The localities of the area of deposit have changed during these land movements, and this change is only one of nature's ways of building up and restoring continental land. Thus, within Pleistocene[1] times the Mississippi and St. Lawrence, and probably the Congo and other great rivers, when in consequence of continental elevation the land areas were increased, have extended further out into what is now open ocean, and the sedimentary deposits there laid down are now concealed by calcareous oozes.

The Continental Margin from Florida Isthmus to Sandy Hook.—On the other hand, during eras of continental depression Pleistocene deposits have been laid down on the present borders of the continents, such as are described by McGee, on the coastal plain of North America, south-east of the Appalachian mountain system, and extending from Sandy Hook to the Florida Isthmus. 'The

[1] 'Pleistocene' and 'Quaternary' are used by me interchangeably.

sequence of events recorded in the coastal plain deposits is one of the changes in the relations of land and water resulting from rise and fall of the continent; with each continental fall the shores advanced upon the land, and the lower hills and plains and river-valleys were sheeted with sediments; with each continental rise the shores retreated, and the rains and rivers attacked the successive sheets of sediments and carved channels, sometimes entirely through more than one formation, and sometimes far seaward of the present shore line; and the continental rise and fall varied from place to place in the coastal plain and from time to time in the course of its history.' [1]

Speaking of the Columbia formation, which consists mostly of beds of loam, sand, and gravel, McGee shows that it represents a 'submergence of the entire coastal plain in the middle Atlantic slope, reaching 100 feet in the south and over 400 feet in the north, with coeval cold long anterior to the terminal moraine period.' [2] There are well-marked terraces in the firm clays, while the more friable sands have assumed a characteristic undulation form. But more important for our present purpose is the statement that 'the direct record of the Columbia formation goes back to an era three, five, or ten times as remote as that to which the Quaternary has commonly been carried,

[1] 'The Lafayette Formation,' *Twelfth Annual Report of the U.S. Geo. Survey*, 1890–91, p. 425.

[2] 'Three Formations of the Middle Atlantic Slope,' *Amer. Journ. of Science*, January to June 1888, p. 386.

while its indirect record extends far into the Tertiary, and affords part of the data required for equilibrating Tertiary and Quaternary time, the data from the deposits being yet lacking.' [1]

The importance of this direct and independent testimony to the length of Quaternary time will be seen on considering the nature and extent of the sedimentary deposits in the present seas.

[1] 'Three Formations of the Middle Atlantic Slope, *Amer. Journ. of Science*, January to June, 1888, p. 465.

CHAPTER VI

THE CONTINENTAL SHELF AND MARGINAL DISTRIBUTION OF SEDIMENT

ONE of the features distinguishing the marginal form of the great ocean basins and their connection with the great continents is the existence of a band of comparatively shallow water, of varying widths, but roughly following the coast lines. Practically it is a sub-aqueous terrace, which forms the connecting link between the continents and the oceans. The British Isles stand upon a platform outlined by the 100-fathoms line, from which there are descents to the Atlantic in comparatively quick grades, the steepest of which I have already mentioned as being in the Bay of Biscay.

But the sub-aqueous terrace can be traced in narrow widths margining the continents almost everywhere, though it may be, and indeed is, often at a greater depth. If we turn to the maps illustrating the paper of McGee's on the Lafayette formation, already quoted, we shall see a very complete set of submarine contours (given in feet) opposite the south-eastern United States.

These are rather remarkable in showing but a

narrow margin corresponding to the 100-fathoms line (600-feet contour) opposite the coast of Florida, a rather steep descent to the 2,600-feet contour, then a broad submarine terrace to the 2,900-feet contour, of a maximum width of about 90 miles, or a slope of only 1 in 1,584.

Then commences a steep descent to the 9,600-feet contour in a distance of 40 miles; but the contours show a considerable variety of form in the ocean floor, for an adequate conception of which the reader must be referred to the original map.

The same map shows the submarine contours in the Gulf of Mexico opposite the Mississippi down to 11,400 feet, which contour is about 300 miles south of the general coast-line. Here again the floor of the Gulf of Mexico exhibits a varied undulating form, in one place showing an exceptional declivity, for in the space of 18 miles there is a fall from 3,000 feet to 10,500 feet, or a grade of 1 in 12·8.

Submarine Forms of Panama Bay.—In a paper on 'The Geological History of the Isthmus of Panama'[1] Mr. Robert T. Hill, describing the submarine forms of the Pacific Ocean border, says that 'the waters of Panama Bay are so shallow that their deepening does not exceed 1 fathom per mile until the 100-fathoms line is reached, nearly 100 miles south of Panama City. This line will almost connect Cocalita Point and Cape Mala, the two points that mark the entrance to the Gulf. Here,

[1] *Bulletin of the Museum of Comparative Zoology, Harvard College*, vol. xxviii. No. 5, p. 170.

however, almost coincident with the abrupt Pacific coasts, there is a gigantic submarine escarpment, plunging off into the Pacific to the depth of 1,700 fathoms or more.'

Mr. Hill goes on, further, to remark on 'the submarine topography of Panama Bay reproducing in a remarkable way on the floor of the Bay the topography of the land, including several deep arterial channels which may have been submerged river-valleys.'

Whatever the amount of the continental elevation may have been which these features point to, the work both of Spencer and Hill leaves a strong impression on the mind that it must have been very great, and much in excess of anything previously inferred.

Mr. Hill also gives some very interesting information relating to the Pacific tides, contrasting their effect with those of the Caribbean shores, where the rise and fall is only about 27 inches at Colon, while in Panama it is 21 feet. 'The impact of this great wave as it beats against the Pacific coast creates a powerful erosive force. The undertow and flow of the tide has also great effect in removing and distributing coastal *débris*.'

Submarine Forms of the Islands of Antigua, Guadeloupe, &c.—In four very interesting communications to the Geological Society of London, Professor Spencer describes the 'Geological Characteristics of the Islands of Antigua, Guadeloupe,

Anguilla, and St. Christopher,'[1] which he has lately been investigating in relation to his well-known views of the former existence of an Antillean continent. He shows clearly that there has been during the Pleistocene period an epoch when these islands and the platforms on which they stand have been upraised not less than 2,500 feet, and probably much more, and that during this epoch the extended land area was subjected to excessive denudation. The submerged platforms preserve traces of their sub-aërial origin, being intersected with valleys forming continuations of existing streams. It is also interesting to find that Spencer detects evidences of minor oscillations of level and equivalent deposits to the Columbia and Lafayette formations of McGee, already described in connection with the geology of the Atlantic slope. All observations the world over go to prove that the level of the land with respect to the sea is undergoing continual change, rapid perhaps as regards geological time, but slow as measured by our time units.

These geographical changes recorded by Spencer also confirm the view that the Pleistocene formations represent a far longer period than was formerly supposed. What the contributions of the land to the sea have amounted to during the Pleistocene, may be appreciated more correctly when we find that in 1880, at St. Kitts, during a local storm lasting three hours, a rain-gauge of 30 inches at

[1] *Q. J. G. S.*, Nov. 1901, pp. 490-543.

Basse Terre was filled and overflowed.[1] 'The terribly destructive effect of such a rainfall is understood, even without adding that the slackened currents deposited in the town from 4 to 6 feet of mud.'

The Great Bank of Newfoundland.—One of the largest sub-aqueous marginal platforms is that known as the Great Bank of Newfoundland. The area here enclosed by the 100-fathoms line is very great, for it not only stretches far out into the Atlantic, but is extended along the coast of North America as far as South Carolina.

Iceland.—Perhaps one of the most remarkable accumulations of sediment, considering its exposure to the Atlantic gales, is that along the southern coast of Iceland. Round the western, northern, and eastern coasts the shores are deeply indented with innumerable fjords and little inlets, but along the southern coast the margin of land consists of low flats and bars of fine sand and mud, brought down by the many rivers and streams that escape from the edges of the great glaciers and snow fields. A contest is continually waged between the Atlantic breakers on the one hand, and the sediment-bearing inland waters on the other. Bars and spits are thus thrown up, behind which stretch long narrow lagoons. For 250 miles such is the general character of the coast-line. Since the Ice Age, so much sand and silt has been carried down that a wide stretch of low land has been gained, and the

[1] *Q. J. G. S.*, Nov. 1901, p. 589.

sea has become so shallow that for long distances only small vessels can approach the coast.[1]

These examples might be multiplied indefinitely. The submarine coastal margins are all but variations of the same features, and it would take too much space to describe and discuss them in detail.

MARGINAL SUBMARINE SHELVES MAINLY SEDIMENTARY

It now remains to consider in what way these characteristic submarine features have come about.

That the continental shelf, lying usually within the 100-fathoms line, is built up of sedimentary deposits may be inferred from the soundings taken over it, showing the bottom to be mainly mud, sand, and other *débris* of the land.

There appears to be a certain plane below which deposition takes place and above which erosion commences. This conditions the form and regularity of the sub-aqueous terrace, and determines that when this plane has been reached the *débris* is swept out and deposited on the seaward border, and thus the terrace grows in width.

At the outward edge of the submerged terrace, if the accession of new material is sufficient in quantity and rapidly deposited, the sub-aqueous angle of repose will be reached. With this development the *débris* washed out from the continent settles upon this seaward slope, and the terrace

[1] Sir A. Geikie's 'Review of Thoroddsen's Geological Map of Iceland,' *Nature*, Feb. 1902, p. 369.

grows in width in a manner similar to the tipping of an artificial embankment.[1]

It is clear from the experience of telegraph engineers that a sub-aqueous angle of repose exists, but it must vary according to the nature of the materials and their degree of saturation.

Mud may approach fluidity at the surface and become more consolidated with depth, until by its own weight it approaches solidity. No doubt these mud deposits vary greatly in horizontal distribution. It is easy now to understand how the sub-aqueous mud-slides, that give so much trouble to cable companies, come about. The materials simply adjust themselves from time to time to varying conditions, determined by the sub-aqueous angle of repose.

We know that strong currents exist in the ocean borders at great depths. These currents are subject, like all currents, to changes of direction, and during these changes they may cut into and undermine even stiff clay and sand. Similar phenomena have already been described as taking place in the Bay of Biscay opposite the coast of Spain.

During the process of submarine terrace-building vertical oscillations of level may take place, such as are recorded in the papers by McGee already referred to. These variations of level would not

[1] In *The Origin of Mountain Ranges*, p. 309, I have said that the deposits at the mouth of the Amazons are 'but a type of what occurs all along the Atlantic coast of both Americas. . . . Marginal flexures may have taken place in the earth's crust, but the Atlantic slope beyond the platforms is probably a slope of deposition.'

destroy the terraces, but add to their number or their variety of form.

Though the areas of rapid deposit lie near the coast-lines, the finer material is widespread, and extends more or less over the general floor of the ocean.

The extent of terracing is governed by the sedimentary volume borne by the rivers, reinforced by materials due to sea denudation, and by the length of time the continents remain stable.[1]

In some cases—but I think they are few—the terrace may be cut out by the horizontal denudation of the sea. Any original inequalities of the submerged border get reduced to the terrace plane by filling up by deposition and cutting down by denudation, but principally by deposition.

The deposit that takes place seaward of the 100-fathoms line must tend to soften and obscure any sub-aërial features that may have been sculptured prior to submergence.

In a great many cases the submarine slope may be the simple expression of the rate of deposition over the respective areas.

If we turn our attention once more to the Gulf of Mexico and the configuration of the marginal deposits laid down by the Mississippi, we may, I think, satisfy ourselves that many of the steeper slopes represent the sub-aqueous angle of repose of enormous masses of sediment there engulfed.

[1] In a paper on 'The Gulf of Mexico as a Measure of Isostacy,' McGee considers that 1 foot of sediment in 333 years is being deposited by the Mississippi in the Gulf of Mexico, mainly in a narrow zone skirting the coast (*Amer. Journ. of Science*, 1892. July to Dec., p. 188).

In investigating the nature and depth of these sedimentary deposits we labour under the disadvantage of only being able to test little more than a surface film. Borings in the estuaries of rivers, however, tell us that the bed-rock-valley is almost universally at a considerable depth below the sea, and is filled up with gravel, sand, clay, mud, and alluvium.[1]

Coasts of Nova Scotia and Newfoundland.—From the outlines of the land bordering the St. Lawrence and the coast of Nova Scotia and Newfoundland it is clear that there has been considerable subsidence, and, as pointed out by Spencer and others, there are evidences of sub-aërially formed features now to be found by sounding over the submerged shelf. Still further north, as pointed

[1] The same feature of a sub-aqueous delta occurs on a large scale in the Black Sea. In the north-western embayment a space or area of about 22,713 square geographical miles is enclosed by the 100-fathoms line stretching from Cape Kaliakra to Sevastopol. This area receives the drainage of the rivers Danube and Dnieper, the average depth being about 30 fathoms. The seaward face of the slope from the 100-fathoms line is comparatively steep, as it is on continental borders. This enormous area, nearly equal to that of England and Wales, is, in my opinion, a plane of deposition, or sub-aqueous delta on a large scale, combinedly built out by the two great rivers. Seaward of this sub-aqueous deltaic slope soundings of 700 to 800 fathoms occur, gradually deepening to a maximum of about 1,200 fathoms at the eastern end of the sea.

Another area of deposition occurs off Kertch Strait, where accumulates the sediment of the drainage from the Sea of Azov, and again the phenomenon is repeated of a shallow sub-aqueous plain and sudden seaward slope. Everything points to the existence of an enormous thickness and area of deposit of sediment, and if, as Suess maintains, the sinking of the Black Sea area is comparatively recent, geologically speaking, what an immense vista of geological time does this not open out? In reference to submarine deltas see pp. 85, 86, 89, and 313, *Origin of Mountain Ranges.*

out by Israel Russell, similar evidences are to be seen. In his own words, 'A glance at the map of North America suggests that the large number of islands into which the land is broken on the north-eastern border of the continent is due to a recent subsidence.'[1]

North-eastern America and Greenland.—Greenland again, a land of gneissic rocks and granite, cut into deep fjords like Norway, speaks eloquently through these orographic features of a former continental elevation. At the same time evidences of post-glacial subsidence and recent elevation are distributed here and there in most of the northern regions. Nansen, after leaving the inland ice, when descending the valley towards Ameralik Fjord, on the west coast, noted, where a land-slip had occurred by the stream, that masses of old mussel-shells were exposed to view, and infers that here again we have proof of geologically recent subsidence and re-elevation,[2] and he is very much impressed with the rapid sedimentary filling up of the heads of these fjords with the glacial clay, mud, and sand brought down by the glaciers.

Spitzbergen and Novaya Zemlia tell the same story, together with the northern coast of Russia and other lands enclosing the Polar Ocean, which is now found by Nansen, contrary to previous opinion, to be a deep-sea basin.

Deposits bordering the Polar Ocean.—Into this

[1] *Rivers of North America*, p. 291.

[2] *The First Crossing of Greenland*, vol. ii. p. 139.

basin pour the spoils of the land brought by the great continental rivers, such as the Mackenzie, the Obi, the Yenisei, and the Lena, with many others of less magnitude. No wonder that the floor of this ocean and the Barents Sea, where so far tested, is found to be all mud or sand.

The Yukon, one of the largest rivers of North America, has a hydrographic basin of about 440,000 square miles, and the volume of the river, though as yet unmeasured, is comparable with that of the Mississippi.[1]

This river does not empty direct into the Arctic Ocean, like the Mackenzie, but into the Behring Sea, which is shallow and without strong currents or tides. The great river has built up a delta perhaps not less than that of the Mississippi. Great quantities of driftwood are carried out to sea, and the sediment that is not used up directly in building the delta is spread over the floor of the Behring Sea.

The Tundra and the Siberian Continental Shelf.—It is unnecessary to go over in detail the characteristics of the other Arctic rivers, but the Yukon may be taken as representative.

There is a vast treeless tract, known as the *tundra*, built up along the shores of the Arctic Sea, a compound of layers of frozen silt and intercalated dirty ice laid down by the rivers.

It hardly needs pointing out that the enormous land areas draining into the Arctic Ocean must

[1] Israel Russell, *Rivers of North America*, p. 285.

contribute great volumes of sedimentary matter, which is being laid down rapidly near to the coast. In this way the rivers are pushing out continental shelves towards the borders of the deep Polar Ocean. There is, however, very little accurate information existent on the subject, from the inherent difficulties of obtaining it. The geographic forms, however, point to a long continuance of similar characteristics, with minor elevations and subsidences, from which we may justly infer that the Arctic Ocean has been a sedimentary basin for a long period of geologic time. Baron Nordenskiöld, speaking of the margin of the Arctic Sea, says that very commonly a uniform depth prevails, amounting near the shore to from 4 to 10 metres, but increasing seawards gradually, and remaining unchanged over very extensive areas. This is caused by the ice-mud-work, which goes on nearly all the year round. He also refers to the discoloration of the water, at one period of his voyage, due to the Ob Yenisei current, which made his vessel appear to sail in clay-mud.[1] Nansen also records in these regions the sea becoming suddenly covered with brown clayey water, which could not be deep, as the track left by the *Fram* was quite clear water; 'it seemed to come from a river farther south.'[2]

Driftwood was met with at Port Dickson, and the river Yenisei floats much driftwood out to sea,

[1] *Voyage of the 'Vega,'* vol. i. pp. 188-89.
[2] *Farthest North*, vol. i. p. 186.

where, getting waterlogged, it sinks, or it is thrown up on the shores of Novaya Zemlia, the north coast of Asia, Spitzbergen, or perhaps Greenland.[1] Nordenskiöld found that the delta of the Lena had undergone much change during a period of 140 years, so that the maps made 140 years ago were useless. Where at that time there were sandbanks there are now large islands, overgrown with wood and grass. At other places whole islands had been washed away by the river.[2]

The chart showing the course of the *Vega* in Nordenskiöld's famous voyage demonstrates that the waters bathing the shores of Northern Asia are very shallow, and that the 100-fathoms line would extend out to a considerable distance. At no point between the Gulf of Obi and Behring Straits, on the track of the *Vega*, are there soundings shown greater than 100 metres, while the majority range from 10 to 30 metres.

Speaking of Behring Straits, Nordenskiöld remarks that an elevation of the land less than that which has taken place since the glacial period at the well-known Chapel Hills at Uddevalla would be sufficient to unite the two worlds of Asia and America with a broad bridge.[3]

As is the case with all the other Siberian rivers running from south to north, the western strand of the Yenisei wherever it is formed of loose earthy layers is also quite low, and often marshy; while the

[1] *Voyage of the 'Vega,'* p. 199.
[2] *Ibid.* p. 367. [3] *Ibid.* p. 243.

eastern strand consists of a steep bank, 10 to 20 metres high, which north of the limit of trees is distributed in a very remarkable way in pyramidal mounds.

Numerous shells of Crustacea found here, belonging to a species which still live in the Polar Sea, show that at least the upper earthy layer of the *tundra* was deposited in a sea resembling that which now washes the north coast of Siberia.[1]

Here, as elsewhere, though perhaps in a more instructive form, we have records of oscillations of level, the last of which evidently has been a small rise of the land.[2]

Probable Existence of Buried River-Valleys on the Arctic Sea Border.—To one who has studied the subject of buried river-channels this area of Northern Siberia and the sea floor beyond presents the sort of conditions that would lead him to suspect the existence of buried valleys channelled out during former continental elevation, and now obscured by a mantle of clay, sand, and mud. Whether borings will ever come to be made in this inclement country that will prove or disprove this theory is very doubtful, but soundings may perhaps in the future tell us more of the floor of the sea than is at present known, and so help the solution of this interesting problem.

Movements of Elevation and Depression in

[1] *Voyage of the 'Vega,'* p. 378.

[2] Nansen says that Rocky Island, off the coast of Siberia, on the Kara Sea, has specially marked shore lines indicating the former level of the sea (*Farthest North*, vol. i. p. 145).

Northern and Central Asia.—Prof. C. F. Wright, one of the latest observers of the physical features of Northern and Central Asia, in a communication to the 'Quarterly Journal of the Geological Society' in 1901,[1] considers there are evidences over large areas of comparatively recent subsidences of the continental land. In Manchuria he finds that the troughs of all the streams are very old, and show a recent depression of the land, resulting in an extensive filling up of the channels.

The Amur, he thinks, has many points of resemblance to the St. Lawrence, and that there are abundant indications that the whole drainage of the Lower Amur basin has been obstructed by a recent differential subsidence. Lake Baikal is completely surrounded by mountains, except at one narrow depression, through which the Angara River carries off its surplus water. Its surface is 1,560 feet above the sea, and its southern portion is 4,500 feet deep, or 2,940 feet below sea-level. Its comparatively recent origin can be inferred from the fact that it is not filled with sediment brought into it by the Selenga and other rivers, all of which have in their long course through the central plateau eroded valleys several miles wide and of great depth. As has been frequently pointed out, it is inhabited by a species of seal closely allied to those found in the Caspian. These facts, together with the terraces recorded at Trebizond, on the Black Sea, at a level of 650 feet, Mr. Wright thinks

[1] Vol. lvii. pp. 244–50.

are indubitable evidence of a former subsidence of continental proportions. We may add that they also show as the latest phase a considerable re-elevation of the land.

Mr. Wright also observes, in relation to the active denudation of the continental surface now in progress, that on approaching the western shore of the Yellow Sea one crosses, 40 miles out, a sharply cut line, on one side of which is clear sea-water, and on the other water turbid with silt gathered by the rivers from the löess-fields of China.

The South-Eastern Coast of Asia, Japan, and the Malayan Archipelago.—From the Ganges along the coast of Burma, Siam, Annam, and China, to the mouth of the Amur and Sea of Okhotsk, numerous great rivers and innumerable smaller ones are carrying their quota of sediment to widen the continental margins, and are building up strata now which in course of geologic time will doubtless become heated, compressed, and laterally folded into great mountain chains.

When we take into consideration the numerous large islands, such as Japan and Formosa, off the Asiatic mainland, and the tropical group of islands forming the Malay Archipelago Sumatra, Java, Borneo, the Philippines, and New Guinea—almost connecting the continents of Asia and Australia, this large area is probably one which, through rapid disintegration of the rocks and the wash of tropical rains, combined with volcanic ejectamenta, is contributing more sediment to the formation of

future continents than any other equal space on the globe.

Wallace, in his classical work on the Malay Archipelago, observes that the expanse of sea which divides the islands of Java, Sumatra, and Borneo from each other and from Malacca and Siam is so shallow that ships can anchor in any part of it, since it seldom exceeds 40 fathoms in depth.[1]

The Philippines and Celebes.—Mr. George F. Becker, in his report on the Geology of the Philippine Islands,[2] calls attention to the many existing evidences of Pleistocene variations of level to be seen in these islands. He says: 'Physical evidences that the islands are rising at the present time, or have been rising within a few years, abound from one end of the group to the other. It is also clear that the amplitude of the movement has been very great.'[3] The recent plains, which form the most valuable and thickly settled portion of the islands, are in large part 'areas of marine denudation and deposition, outer portions of the continental plateau, which have been lifted above the water-level in very recent times.'[4] Further evidences of movement are recorded in the 'high terraces, as well as low ones, abundant throughout the islands. . . .' This part of 'Cebu must approach 2,000 feet in

[1] P. 9, 1890.

[2] *Twenty-first Annual Report of the U.S. Survey*, 1899–1900, Part iii., 'General Geology *re* Phosphate Deposits, Philippines.'

[3] *Ibid.* p. 77.

[4] *Ibid.* p. 76.

height, and is scored by a vast number of terraces, all of which are sensibly horizontal.'[1]

These islands, it may be here remarked, 'lie along the edge of a vast submarine precipice, or, in other words, at the very abrupt limit of the continental plateau.' Elsewhere Mr. Becker speaks of the eastern edge of the continental plateau being outlined by submarine cliffs, and a reference to the map (Pl. LXVII.) will show that the plateau on which the islands are based is not submerged more than 50 metres. Doubtless this has been largely built up by sediment. It is very instructive to see that there are 'deeps' in this plateau, one of which reaches no less than 3,291 metres, proving plainly, what is here so frequently insisted upon, that the sea bottom is very diversified in its topography. Nor must we lose sight of the fact that the base rock of the Philippines consists of crystalline schists, notwithstanding that volcanoes abound, so that there would be less justification here for the usual explanation, that such irregularities of bottom are due to volcanic action.

Celebes, an island lying due south of the Philippines, exhibits signs of recent elevation and depression. A raised beach can be traced at heights of 90 feet above sea-level, and in other places submerged forests are found.[2]

[1] P. 79. [2] *Nature*, May 1902, vol. lxvi. p. 3.

CHAPTER VII

THE DEEPS OF THE OCEAN

WITH the increase of soundings the oceanic floor assumes an unsuspected variety in its form and configuration.

In studying the 'deeps,' which are concave depressions or troughs sinking down *below* the general floor-level of the basins in which they occur, this result of recent investigations must be kept steadily in view.

The forms or prominences rising *above* the general oceanic floor-level approximate to land forms, and lend colour to the view that the Mid-Atlantic was once an extensive and diversified land area.[1]

It seems indeed to be travelling out of the path of probability to assume that the Atlantic submerged ridges and the varying gradients of the Atlantic bottom are due to submarine volcanic action alone. No facts of geology lead us to draw such a tremendous inference. It will be shown before the completion of this chapter that the cubic content of these submerged ridges is

[1] See 'Contributions to the Study of Volcanoes' (Judd) (*Geo. Mag.*, 1876, p. 531).

enormous, and they are probably as enduring features of the globe as either oceans or continents. There is an ever-increasing mass of evidence pointing to a former greater elevation of the continents than what now exists.

Dr. Spencer has done much good pioneer work in this direction; and, though we may not be able to adopt his views to their full extent, he has certainly aroused the geological world to unsuspected possibilities. In connection with this question the discovery by the Deep-Sea Sounding Expedition already mentioned of the *Libbey Deep*, in lat. 40° N., long. 63° W., 3,318 fathoms; the *Sigsbee Deep*, in lat. 40° 11′ 51″ N., long. 59° 51′ 30″ W., 3,045 fathoms; and the *Thoulet Deep*, in lat. 40° 25′ 30″ N., long. 58° 52′ W., 3,160 fathoms, is most interesting. These profound depths of the ocean exist within a distance of about a couple of hundred miles of the 100 fathoms platform on which Nova Scotia stands, the bordering continent being a very old land area long free from volcanic phenomena.

As facts accumulate we may expect these rigid views as to the unplastic character of the configuration of the sea floors to be rudely shaken.

Distinction between Sub-Oceanic and Land Forms.—So far as we at present know, one of the main distinctions between the form of the ocean floor and that of the continental land is the existence in the oceans of great depressions, or 'deeps' as they are generally called.

Dana has treated of these, but necessarily in an incomplete manner.[1] The method he adopts is largely that of inferences from analogical geographic forms, and the conclusions he comes to are mostly of a negative character.

In considering these continental and oceanic problems it is necessary to remember that the oceans occupy nearly three-fourths of the total area of the surface of the globe and the land but a little over one-fourth.[2]

The continents are thus protuberances rising through the waters. It follows as a geometrical truth that, the volume of the ocean waters remaining constant, the larger the area of land at any given period the deeper must have been the oceans.

That is to say, the average depth must have been greater with every continental accretion and shallower with every continental reduction, always assuming that the surface area of the globe does not vary.[3] If we look upon the continents as protuberances upon the spheroid and the oceans as depressions beneath it, we shall, in my opinion, take a correct view of their relations. If, further, the continental protuberances are due to increase of volume without variation of mass, as suggested in

[1] 'On the Origin of the Deep Troughs of the Oceanic Depression: Are they of Volcanic Origin?' (*Am. Jour. of Science*, 1889, January to June, p. 192).

[2] Herschel gives the dry land as 51 million square statute miles, and 146 million square miles as the extent of surface occupied by the ocean.

[3] Some geologists maintain that the volume of water fluctuates.

Chapter I., and the ocean basins are due to decrease of volume without variation of mass, we have an instrument of investigation that will enable us to unlock some of the mysteries of the genesis of the larger geographic forms.

Ocean Basins and their Deeps.—The ocean floor, according to most oceanographers, is divided into separate irregularly shaped basins, which, when in the proximity of land, bear decided relations to the bordering continents. Within these large basins here and there subsidiary basins, or the so-called 'deeps,' have been proved to exist, and their numbers are being added to year by year as the abysmal depths of the ocean are explored by the sounding line. Among the deepest of these troughs is one off the north-east coast of Japan of 4,655 fathoms, found by the United States s.s. *Tuscarora*; another of 4,475, south of the Ladrone Isles, discovered by the *Challenger*; and one of 4,561 fathoms, north of Porto Rico, found by the United States steamer *Blake*. Later, the *Egeria*, under the command of Captain Aldrich, R.N. (1888), during a cruise and search for reported banks to the south of the Friendly Islands, obtained two very deep soundings—one of 4,295 fathoms, the other 4,430 fathoms; the latter in lat. 24° 37′ S., long. 175° 8′ W.; the other about twelve miles to the southward. 'These depths are more than 1,000 fathoms greater than any before obtained in the Southern Hemisphere.[1] The greatest depth yet

[1] *Nature*, November 8, 1888, p. 39, vol. xxxix.

discovered is in the Moser Basin, so named by Alex. Agassiz. The U.S. exploring vessel *Hero* got a sounding of 4,475 fathoms near the island of Guam, which record was soon after broken by the U.S. Fish Commission steamer *Albatross* with another in the same basin reaching 4,813 fathoms.

In the Tonga-Kermadec Deep, near Tongatebu, the *Albatross* got a sounding of 4,540 fathoms.

These depths are statements of actual facts which are recorded on the Admiralty charts. Oceanographers naturally go further than this and attempt to give approximations to the size and form of the basins in which they are situated.

There is a good deal of divergence as well as a rough general agreement in the hypothetical contours of these great depressions in the ocean bottom. I shall not myself attempt to give one more version, and will wait patiently for further knowledge. In the meantime I refer my readers to the '*Times* Atlas, 1898,' Charts V. and VI., to show what is inferred as to the extent and form of the 'deeps' of the North and South Atlantic Oceans.[1] The following is a descriptive account:—

Commencing with the South Atlantic, we have opposite the mouth of the Rio de la Plata, at a dis-

[1] In addition to the Admiralty charts in which the soundings are given without inferential contours, the following are among the bathymetrical charts that may be consulted: *Thalassa*, by John James Wild, Plates ii. and iii., 1877; 'On the Origin of the Deep Troughs of the Oceanic Depressions: Are they of Volcanic Origin?' by James D. Dana (*American Journal of Science*, vol. xxxvii., 1889, plate vii.); Bathymetrical Chart of the Oceans, showing the 'Deeps' according to Sir John Murray (John Bartholomew & Co., Edinburgh).

tance of about 300 miles from the coast, an unnamed deep of 16,802 feet, and further east a larger trough of 18,400 feet, and about halfway between Buenos Ayres and Cape Town another of 16,969 feet. Then off the coast of Brazil we have a trough of very large area having a depth of from 16,000 to 19,000 feet, but east of the island of Trinidad there is a sounding reaching 19,705 feet. Due east of this is a large depression called the West African Basin, about 400 miles from the mouth of the Congo.

There is a small area of depression about 500 miles east of the mouth of the Amazons having a depth of 17,878 feet.

Then it is not till we get to the West India Islands that any great depression is met with, but here the area of depression is very great, and in the Virgin Islands Deep reaches a maximum of 27,366 feet and in the West Indies Deep 22,950 feet. This portion of the globe is a great area of depression, and it is here that Dr. Spencer finds his great continental problem, already to some extent discussed. North of this another great area of depression called the North Atlantic Basin, with a very deep subsidiary depression about 600 miles south-east of Boston, reaching 22,200 feet, and two others, one occurring between New York and the Canary Islands, of 21,000 feet. On the eastern side of the North Atlantic is a very long irregular trough following roughly the African coast-line called the Cape Verd Basin, joining on to another irregular trough which curves round towards

the east and roughly follows the land forms or trend of the African and Spanish coasts. The soundings are from 16,000 to 18,000 feet, with two small depressions of nearly 20,000 feet. Finally, there is the Eastern Azores Trench, midway between Lisbon and Halifax, having a depth of over 17,000 feet.

As showing how small is our knowledge of the ocean bottom, the preceding lines had not long been written before it was announced that the *Belgica* expedition had discovered a depression of 4,040 metres to the south of Staten Island (South America) and a continental plateau extending south of the 70th parallel, both discoveries tending to confirm the principles laid down in this memoir.[1]

Of the Polar Sea we have less information than of the Atantic, but Dr. Nansen has proved that it is a great basin of depression north of Spitzbergen and north-east of Franz Josef Land of depths varying from 3,300 metres (1,800 fathoms) to 3,900 metres (2,100 fathoms) in the deepest places sounded,[2] and this is an area that was supposed to be under 100 fathoms, a mistaken generalisation from a previous sounding of 80 fathoms by the *Jeannette*.

Position of Deeps governed by Trend of Coast Lines.—Broadly speaking, it is noticeable that the position of these oceanic troughs seems to be governed by the trend of the continental coast lines. Instead of being arranged along the central areas of the South and North Atlantic, we find this

[1] *Nature*, July 4, 1901, p. 238.

[2] *Farthest North*, vol. i. p. 410.

central space occupied by more or less continuous ridges, their alignment being also apparently related to the trend of the coast lines of the continents on either hand. These ridges and plateaux lie generally from 8,000 to 12,000 feet below the surface of the ocean, and form the base upon which the oceanic islands such as the Azores, St. Paul, Ascension, and Tristan d'Acunha stand. There are also submerged peaks and shoals which with further soundings would no doubt be vastly increased in number.

It is noticeable in the North Atlantic that the outlines of the 'deeps' and the configuration of the bottom and of the plateaux seem related to the general form of the oceanic basins and the surrounding continental land forms.

We have seen that careful and frequent soundings, such as were taken under the supervision of Mr. Peake, between Great Britain and the Azores to North America as already described, result in showing that these contours of the ocean beds, though approximating to the general actual form of the ocean floor, are very far from discovering to us the serried configuration which really exists.

RIDGES AND PLATEAUX OF THE NORTH AND SOUTH ATLANTIC

Looking at the forms broadly, and omitting the innumerable minor breaks and changes of gradient, I think an impartial examination of the information conveyed by the soundings will prove that, area

for area, in the North and South Atlantic there is a greater diversity of form, with greater heights and greater deeps, below the oceanic waters than in the surrounding continental land.

Roughly speaking, in round figures, we may say that the ridges and plateaux already described lie from 8,000 to 10,000 feet above the great oceanic troughs such as the Brazilian and North Atlantic Basins and 12,000 above the greater depressions or deeps. This elevation approaches very closely that of the Tibetan plateau of Central Asia and the South American high plateau, while, if we take the Azores and similar islands as the summits of probably submerged mountains, whether they be volcanic or otherwise, we find peaks of an altitude to rival the Himalayas.

One of the latest attempts to estimate the mean heights of the continents above ocean level is that by Sir John Murray, the results being as follows :—

Europe . . .	939 feet
Asia	3,189 ,,
Africa . . .	2,021 ,,
North America .	1,888 ,,
South America .	2,078 ,,
Australia [1] . .	805 ,,

It seems pretty obvious that if we drew a plan at the 18,000 feet bathymetrical contour line to

[1] 'On the Height of the Land and the Depth of the Ocean,' read before the Royal Society of Edinburgh, December 19, 1887.

represent a hypothetical retreat of the waters to that level we should have in the present oceanic areas of the Atlantic a central strip of land following the now submerged 'ridges' which would equal at least in mean height that of the continent of Asia.

It is not my intention to follow up this description of the configuration of the Atlantic bottom with one of the Pacific.

The area is so very much larger, and the amount of information so much less, that any attempt to portray the sub-oceanic forms would be in the highest degree hypothetical. Deeps exist in it, some of which I have mentioned, and in some cases these are situated close to the land, as in the ocean trough east of and parallel with Japan.

CHAPTER VIII

CONTINENTAL PROMINENCES NOT THE RESULT OF FAULTING

IT is held by some, notably by Suess, that the ocean basins are the result of subsidence by faulting, and that the continents owe their prominence mainly to their foundations having remained solid through earlier consolidation.

In none of the features of the Earth which I have so far described is there evidence pointing to faulting on such a scale as is required by this hypothesis.

If it were true that faulting has outlined the continents, surely some striking evidence of its existence would have been discovered on some of the continental margins. On the contrary, as previously pointed out, the marginal features are more often traced out by mountain chains.

Then again, so far as our present knowledge of 'deeps' enables us to judge, they are simply concave sinkings in the ocean bottom such as would be produced by the shrinking of matter of the Earth in a *focus* below the concavity. Such depressions, though more strikingly frequent in the submerged portions of the crust, are not altogether

absent on the continental land. The Mediterranean Sea, the Black Sea, the Dead Sea, the Caspian depression, and Lake Baikal are analogous if not precisely parallel phenomena.

LARGER FEATURES OF THE EARTH NOT DUE TO DIFFERENTIAL SHRINKING OF THE SPHEROID

The more the geological phenomena and geographical features of the globe are studied, the more difficult it becomes to explain them by a differential radial shrinking of the spheroid. Their complexity is too great to admit of so simple an expedient leading to a true solution. In the first place, as I have frequently pointed out—and no one, to my knowledge, has attempted to deny it—the crust of the globe is proved by geological evidence of the most convincing nature to have been subjected at times to compressive, and at other times to tensile stresses. This has happened again and again, over the same areas, the compressive stresses, producing folding and overthrusts, the tensile normal faults.

The shrinking-globe theory only provides continuous compressive forces and gives us no explanations of normal faulting; nor has such an explanation, to my knowledge, been formulated.

Again, if what I have already written is a true account of the continental and subaqueous phenomena as exhibited in raised beaches and drowned valleys, the continental land has been subjected at one time to absolute elevation and at

another to absolute depression—that is, it has oscillated about a mean spheroidal figure. According to the theory which ascribes ocean basins to successive differential subsidences, there have been no regional uplifts, the elevations of the Earth on a continental scale being merely relative. This appears to be the view taken by Suess. If this be so, how can the existence of former elevation and former depression in the same land area, of which we have the most convincing evidence, be explained?

According to the views advocated in this work, not only have there been repeated continental uplifts and depressions, but the ocean floor, with its irregular loads of sub-oceanic ridges and peaks, has also been subject to vertical oscillations of level. These fluctuations bring into action a disturbing element as regards the sea-level in relation to that of the land. If at any time a regional rise of the ocean floor took place without a compensating depression elsewhere, the sea would gain upon the land. If, on the contrary, a depression of a large area of the sea bottom set in without an equivalent rise elsewhere, the ocean waters would be drawn away from the land. It is this that complicates the consideration of the relation of the land to the ocean. There is, however, this difference to be kept steadily in view: the effect of a rise or fall of the oceanic waters would be world-wide and affect more or less the whole marginal land of the globe.

May not this, as already suggested, help us to

account for the drowned river valleys, the evidences of which seem to be so universal? The hypothesis that in the main the irregularities and configuration of the Earth both above and below the ocean are altogether due to differential subsidences labours under this serious difficulty. We have seen that there are unmistakable evidences, in the existence of marine terraces from the present sea-level to over a thousand feet above it, of the former presence of the sea. To account for this phenomenon on the subsidence theory we have to postulate a subsidence of the ocean bottom or of part of the continental land on a truly colossal scale. The whole of the oceanic waters have to be withdrawn into these chasms, that the terraces may appear at the levels we see them; and when we consider that drowned river valleys are an even more universal phenomenon than marine terraces, we have to assume that the oceanic waters at a former period were even at a lower level than at present. Thus, to account for these phenomena we require at least two enormous disturbances of the oceanic level; but here our difficulties only begin, because geology tells us that these phenomena have been repeated times without number.

That no such disturbances have taken place we may feel assured. Nature works with economy and not in this extravagant fashion. The enormous contraction of the Earth's radius which is involved in this theory has, I think, not been considered. Of such a contraction we have no evidence either

inductively or deductively. In this connection it will be well to read Chapter XI. of the 'Origin of Mountain Ranges,' which will render it quite unnecessary to repeat the arguments here. It will be seen that the theory advocated in this work—viz. that of regional subsidences and elevations—involves a much less expenditure of mechanical energy.

BOOK II

DYNAMICS OF MOUNTAIN STRUCTURE AND EXPERIMENTAL GEOLOGY

CHAPTER IX

CHANGE OF FORM BY EXPANSION AS AN ELEMENT IN MOUNTAIN BUILDING[1]

INTRODUCTION.—Some critics of my theory of the origin of mountain ranges appear to have strangely lost sight of one of the essential principles upon which it rests. This seems to have arisen through their attention having been too much engrossed with what I have said on the subject of cubical or voluminal expansion.

In my original work it was shown that the calculations of the earlier investigators on the vertical lifting of a given thickness and area of the earth's crust by a given rise of temperature must be multiplied by three to arrive at a correct result, as they omitted to consider the expansion in two horizontal directions at right angles to each

[1] The substance of this Chapter appeared as a communication to the *Geological Magazine* in August 1896.

other, confining their attention to linear expansion in a vertical direction. But, while calling attention to this oversight, it was certainly not my contention that expansion would affect the strata of the earth's crust like the expansion of water in an inexpansible vessel.

The effect of expansion on a solid material like the earth's crust by differential heating is to set up stresses and strains, which relieve themselves in the direction of least resistance, and in doing this internal movements and change of form ensue. A change of volume of a section of the earth's crust therefore involves internal movement, distortion, and change of form.

This principle I illustrated by experiments on the ridging up produced by heating sheets of lead; by the distortion of a sheet of zinc and a sheet of iron, riveted together, and placed in an ordinary oven; by the well-known wrinkles and folds that occur in lead gutters, lead-lined baths and sinks; and since, by the permanent expansion which frequently takes place in terra-cotta copings of walls through differential heating by the sun's rays.[1] I could add considerably to this list, but it is sufficient for my present purpose. The ridgings up and distortions were shown in all these cases to be the cumulative result of compressions and tensions set up by successive expansions and contractions, due to alternations of temperature, ending in the case of a sheet in a permanent extension, which

[1] *Geol. Mag.*, Dec. III., vol. v. pp. 26, 27.

is compensated for by folds or wrinkles, or in the case of a bar by lengthening.

Applying this principle to the probable effect of changes of temperature in the earth's crust, I showed that the strata of which mountain chains are composed have a wide areal extension, as in the plains of Russia, which are composed of the Silurian and other rocks involved in the Ural uplift. The same principle was shown to hold true with the Appalachians; and later I have called attention to British geology as teaching us a similar truth.[1] These strata, out of which mountain ranges are evolved, may be considered as wide and extended sheets almost paralleled on a small scale, excepting for the variable thickness distinguishing geologic deposits, by the sheets I experimented upon.

Without, however, considering the *causes* of changes of temperature in the earth's crust—which I have treated of elsewhere—it is sufficient for our present purpose to accept the fact that profound changes do occur. Beginning with Hutton and Playfair, I can think of no geologist who has written upon the subject of metamorphism and geological dynamics who fails to call to his aid, in some form or other, the effect of heating upon the sediments which compose the rocky covering of our globe.

If it be assumed, then, that a great sheet built

[1] 'British Geology in relation to Earth-folding and Faulting' (*Geol. Mag.*, Dec. IV., vol. ii. pp. 557–65, reprinted in this volume).

up of various strata of sedimentary origin, with, perhaps, intercalations of ashy and igneous beds, combinedly reaching in places thicknesses measured by miles, is by slow degrees subjected to fluctuating increases of temperature, it is evident from the illustrative experiments referred to, and the further experiments to be detailed, that not only a linear vertical expansion will ensue, but that the horizontal expansion, as much greater in proportion as the areal extent is greater than the thickness, will produce, by small increments and minor alternations, a creep, ending in an anticlinal fold in a position determined by several conditions.

It is thus seen that an actual movement or displacement of material proportionate to the amount of expansion has taken place.

The excess of the material of this compound sheet, or what may be called the strata-plate, over the space it originally occupied is disposed of by a heaping up, by folding, along a line of maximum pressure or least rigidity. In this way a permanent feature in the form of a fold is built up upon the earth's crust, which may be increased in amplitude by future expansions, but which will remain unaffected by any succeeding contractions of the strata-plate.

Let us now consider how a deficiency caused by a contraction of the strata-plate can be compensated for. I have shown in my original work that it may be met, in the case of small contractions, by what I have called compressive ex-

tension, which is a lengthening of the strata by compression due to the weight of the overlying strata going on *pari passu* with the contraction due to a fall of temperature; so that, instead of separating by fissuring, the strata are made to continually occupy the same superficial horizontal space, while, at the same time, becoming thinner by compression. As the greatest expansion takes place at the base of the deposits, or in the underlying crust, there is, in most cases, a load sufficient to act upon and mould the contracting bed, and in this way convert horizontal into vertical contraction, the rigidity of the strata and power of conveying lateral thrust being at the same time preserved. Therefore, in the case of a general rise of temperature of the strata-plate, but with minor fluctuations and falls of temperature, the effect of every rise, however small, will—whether the compensation be by compressive extension or by minor faulting and keying up—tend to still further lengthen the strata and develop the anticlinal fold, or to add to it other parallel anticlinals, until a complete folded range is finally formed; or, as in the more extreme cases, such as the Alps, a central core or a series of ellipsoidal domes of gneissic rocks is forced up from below, throwing back the folds in fanlike form, and further compressing them. Certain secondary effects may follow, such as the folding and formation of foothills by the gliding of the upper beds down the sloping flanks of the older beds; but it is un-

necessary for me to dwell upon them here, as my object is to enable those who have not yet done so to grasp the idea of successive cumulative expansions, as I conceive them to have acted in the building up of mountain chains.

But these expansions, caused by a general but fluctuating rise of temperature, diminish, and finally cease, by the dying out of the cause producing them. In the absence of compression no more folds are initiated, nor is the amplitude of the old folds increased. Meantime the ever-active elements in the form of air and water are busy at work, reducing and carving out of these folds, domes, and ridges the mountain forms and scenery we are familiar with. Thus the cycle of change is completed, and the broken-up rocks are returned as detritus to the sea.

A general but fluctuating fall of temperature now sets in, and the rocks composing what I have called the strata-plate contract. This contraction can only be met in one of two ways—either by stretching or fissuring. In their nature rocks are incapable of stretching by tension, excepting it be in a very minor degree, and compressive extension could only partially compensate for the profound changes of volume which take place. No doubt the strata-plate will be eaten into, and underlain to a considerable extent, by semi-molten matter in a plastic condition; and the shrinkage of this, combined with the regular shrinkage of the non-homogeneous material of the sedimentary strata,

must inevitably initiate fractures. These fractures, we know, take the form of two series of normal faults, each of which has a more or less definite direction and parallelism, and are classified as strike or dip faults, accordingly as they roughly follow the strike or dip of the strata. The voluminal contraction of the strata-plate is met by the sinking of wedge-like blocks of strata along and between these lines, or, rather, shear planes called faults, and the earth's crust thus remains solid by keying up. In adapting themselves to the voids these blocks are continuously or intermittently sinking, and certain secondary folding along a large fault often occurs. This is fully explained by the fact that the strata nearest to the earth's surface shrink least, so that the wedge, in adapting itself to the void below by sinking, is often in compression in the upper layers, which is met by the turning up of the edges of the strata against the fault-plane.

That this succession of events takes place in nature can be readily settled by reference to any typical section of a mountain range, or to any of the numerous sections taken through the folded regions of Britain published by the Geological Survey.

This latter, it is almost needless to say, constitutes evidence of the best kind, as the authors were simply recording facts, having no thought in their minds of the theoretical relations here expounded. The posteriority of normal faulting

to folding has been remarked upon by even so early an observer as Playfair.[1] Every section I have seen shows normal faults cutting and displacing the folds where the faults and folds exist together, even in the case of those longer undulations into which the strata involved in the folding of the mountain range graduate in those great areas and plains flanking the range proper, which constitute a large proportion of what I have termed the strata-plate. May we not justly infer from these phenomena that normal faulting on a large or general scale never *precedes*, but invariably *follows*, folding, except in the case of previously faulted and folded strata involved in the general compression and uplift? Another feature distinguishing folded regions, such as Scotland, is the prevalence of enormous strike-faults, showing that normal faulting and folding, though arising from movements in opposite directions, are yet closely related.

This short re-statement of some of the leading principles of my theory of the origin of mountain ranges seems necessary in view of certain misconceptions which have arisen, doubtless due to the complexity of the subject. It will also enable the reader to grasp the bearings of the experimental illustrations detailed in the following chapters.

[1] *Illustrations of the Huttonian Theory*, pp. 62, 63.

CHAPTER X

A FURTHER ILLUSTRATION OF THE STRATA-PLATE AND THE CUMULATIVE EFFECT OF SMALL RECURRENT EXPANSIONS

IN 'The Origin of Mountain Ranges' (Plate VI., p. 28) a reproduction of a photograph of a fold in the bottom of a lead-lined butler's pantry sink is given as illustrating the cumulative effect on a plate of metal of the differential expansion caused by changes of temperature. These changes of temperature have arisen from the hot and cold water used in ordinary washing up, for which such a sink is provided, and the sink had in no way been interfered with for other purposes.

Since this fold was photographed in 1886 from a plaster cast of the bottom of the sink, the cumulative expansion continued, until the inner concave side of the fold became tightly compressed against the opposite and outer side of the fold. It thus became an overfold, leaning towards the centre of the sink, as in the accompanying figure, and a second fold began to rise in crescentic form at the opposite side of the sink.

In process of time these continual minute

movements and strains eventually cracked the lead of the fold, and a plumber, being called in to repair the sink, cut the fold out, beat down the lead to a flat surface, and soldered up the gash. Unfortunately I neglected to note the date of this operation, thinking that the sink would now have no scientific value; but it must have been about eight years ago.

So far from the interest being exhausted, it became greater, for a new fold began in course of time to rise on the site of the old one, involving in the movement the soldered joint, which was lifted and twisted without fracture. Several repairs and solderings of further cracks in the fold took place, until I was satisfied that the sink bottom was beyond further repair.

I now had a plaster cast made of the whole area of the bottom of the sink, measuring 23 inches by 14 inches, and from this Plate III. was photographed. Then I had the lead lining carefully taken out without disturbing the form of the bottom, and had the underside photographed and reproduced in Plate IV.

Furthermore, I cut out the large fold from the bottom (fig. 1, Plate V.), to reduce it to a convenient size, and dissected it with a saw along the lines *a b* and *c d*. These three sections were nailed to a board and photographed (fig. 1, Plate V.) in connection with the plaster mould (fig. 2) from which the original Plate VI. in 'The Origin of Mountain Ranges' was taken, and these,

being to the same scale, enable us to see the precise difference between the first fold and the second fold.

The section disclosed by the saw-cut along the line *a b* is especially interesting from the development of overfolding which had taken place, and as showing the thinning out the metal of the unfolded part of the sink had undergone. It is well shown in Plate VI., which gives the section of the overfold natural size.

The lesson taught by the history of this lead vessel is extremely interesting, and shows very clearly that successive changes of temperature, however small in amount, eventually strain and distort a plate in such a way that the recurrent internal movements in time force up a fold bearing a striking resemblance to a mountain range.

Not only does this happen, but we see by the section at *c*, Plate VI., that the movement in the centre part of the fold—that is, between the two ends—was continued so as to produce an overfold, a characteristic feature in the structure of mountain ranges.

A careful consideration will also convince us that this movement, or internal strain, satisfied by the rising of the lead in the form of a fold, is due to *differential* heating of the plate. This is shown by the rate of movement being differential also. The greatest expansion has taken place in the direction of *a* to *b* (fig. 1, Plate V.), along the line of section shown natural size in Plate VI., where

the overfolding has developed itself in a remarkable manner. Plates III. and IV. also show that the movement was to a certain extent radial, which accounts for the crescentic plan of the fold, further evidenced by the lower and minor folds at the opposite corners in the same illustrations. The spreading out and bifurcation of the terminations of the crescentic fold are partially, I think, due to the expansion of the plate on the convex side of the fold.

An examination of Plate V., which enables a comparison to be made between the original fold (fig. 2) and the second fold (fig. 1), shows how this bifurcation commenced with a spreading out of the fold in semi-domical terminations. The history and development of the final form of the fold is recorded well in these series of photographs.

It is very instructive to contrast the perfect moulding and harmonious curves of the original fold (fig. 2, Plate V.) with the gnarled character, irregular ridges, and terminal bifurcations of fig. 1, so representative of the folding of an old mountain range.

Not less interesting to the student of mountain building is the fact that the second fold (fig. 1, Plate V.) rose up on the site of the original fold (fig. 2). A comparison of the two figures will show how closely the second fold followed the lines of the old one, its irregularities arising from the greater stiffness of the portions locally soldered, these solderings rising with the rest of the ridge.

Not only so, but the metal by repeated small strains became less ductile, and so from time to time cracked. One of these cracks or fractures is seen in the photograph (fig. 1, Plate V.) on the inside edge of the ridge, along its central portion.

A measurement of the folding and overfolding along the line *a b* (fig. 1, Plate V.) shows that the sheet of lead expanded along that line 0·63 inch, and the ridge rose 0·46 inch above the bottom.

This is the maximum lengthening that took place since the initiation of the second fold; but for the actual lengthening of the sheet of lead forming the bottom of the sink from first to last must be added the expansion that produced the first fold, estimated by me at about 0·5 inch, making the total 1·13 inch.

It is therefore mathematically demonstrable that this expansion-creep must be accompanied by a *thinning* of the sheet of lead, and a transference of lead to the site of the fold.

In sawing through the sink bottom it was found that in the centre part the sheet was obviously thinner than in and near the folds. Some of this diminution was, no doubt, due to extra wear at that particular spot, but in part also to the thinning by expansion over the central area. The whole plate of lead, including the portion forming the folds, must also have lost thickness not only by wear, but by long-continued chemical action of the water.

The fact that the edges of the plate were

soldered to the lead sides, and the whole fitted into a wooden case, has no doubt influenced the final form the folds have taken; but the differential expansion produced by the hot and cold water falling near the centre of the plate has, in my opinion, influenced it more.

In applying this lesson to the problem of mountain building I wish to point out that the lead bottom represents what I have called a *strata-plate*, forming a differentially heated area of a portion of the earth's crust. Outside this area occurs the unheated and rigid framework, which may be paralleled by the rigid sides of the sink, excepting that in the case of the strata-plate the graduation from the heated area to the outside rigid framework is less rapid.

If we conceive the strata-plate as made up of alternating thicknesses of sheets and beds of rock, overlapping here and dying out there, and bear in mind that we cannot in what is called a laboratory experiment introduce effectively the element of gravitation, which plays such an important part in guiding and limiting earth movements, this record of the history of the lead plate will enable us to grasp the principle on which I believe the ridging up and modelling of the earth's crust has proceeded.

The effect of denudation in our object-lesson has been absent. This, as every one conversant with geology knows, is one of the most important agents by which the earth's crust has reached its

EXPLANATION OF PLATES

PLATE III. View of bottom of lead sink, showing the final ridging up, or second fold, due to small recurrent changes of temperature. Photographed from a plaster cast, size 23 inches by 14 inches.

PLATE IV. Under side of the above, or reverse of that shown in Plate I

These two Plates are placed consecutively for comparison. The corresponding parts, it must be remembered, are necessarily reversed. No. III exhibits the folds in relief; No. IV the concave undersides of the folds (photographed direct from the bottom of the lead lining of the sink after it had been taken out of its wooden case).

PLATE V. Fig. 2 is a photograph from the plaster cast of the original fold. This is the same as Plate VI and The Origin of Mountain Ranges.

Fig. 1 is a view of the second fold to the same scale as Fig. 2. It was photographed from the unpressed metal.

These two figures illustrate the contrast of the symmetrical modelling of the original fold and the irregular rumbling up of the second fold, together with the lifting end tension of the longitudinally soldered joint.

The two lines *a* to *b* and *c* to *d* show the direction of saw cut.

PLATE VI. This is a photograph of the section along the saw-cut line *a b*, Fig. 1, Plate V, showing the structure of the fold and overthrust. The section is as near as possible natural size, but the continuation of the fold is in perspective, and of course foreshortened.

The overlap of the lead plate can be plainly distinguished, the thickening to the right hand [illegible]

PLATE III.

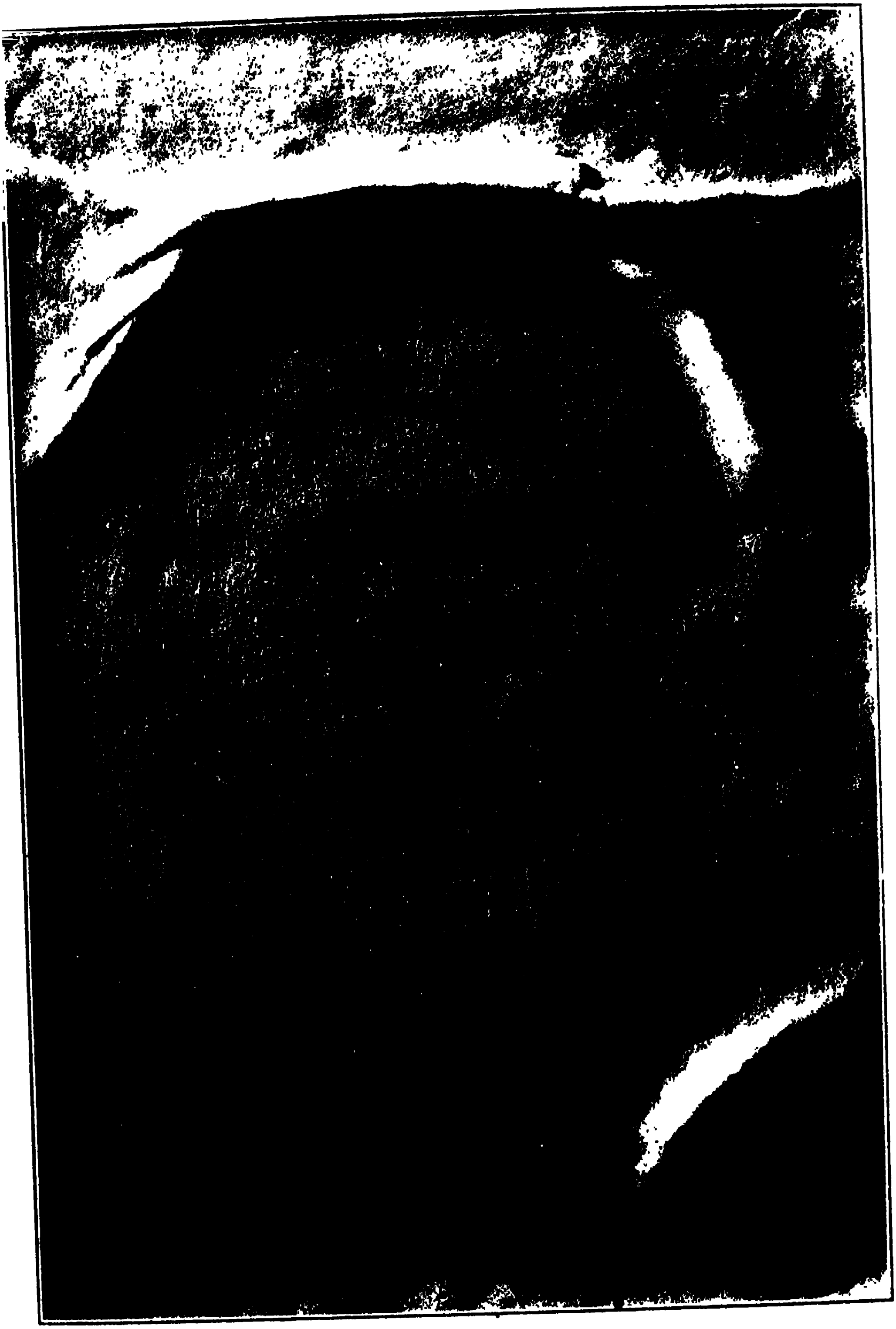


PLATE IV

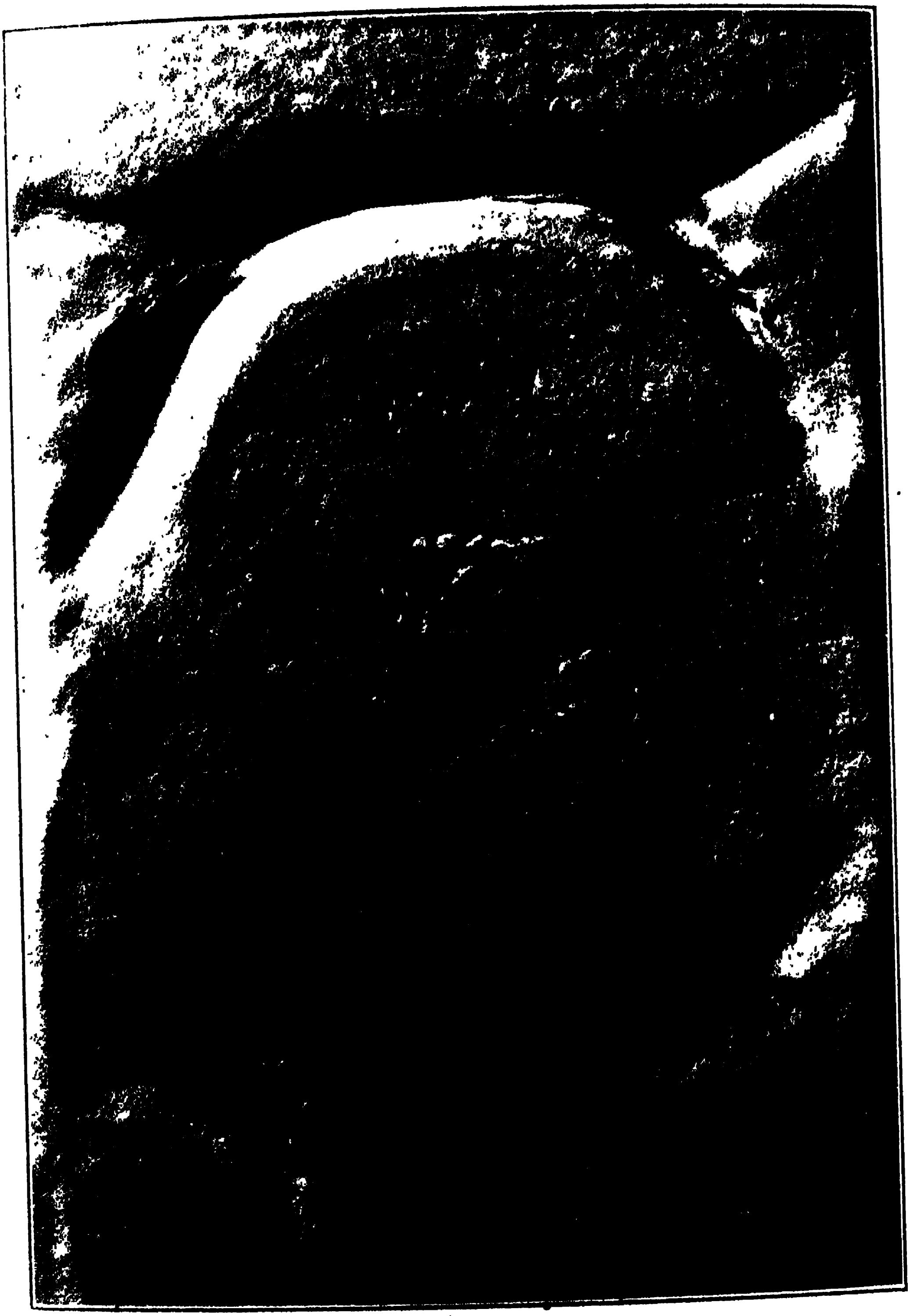


PLATE V

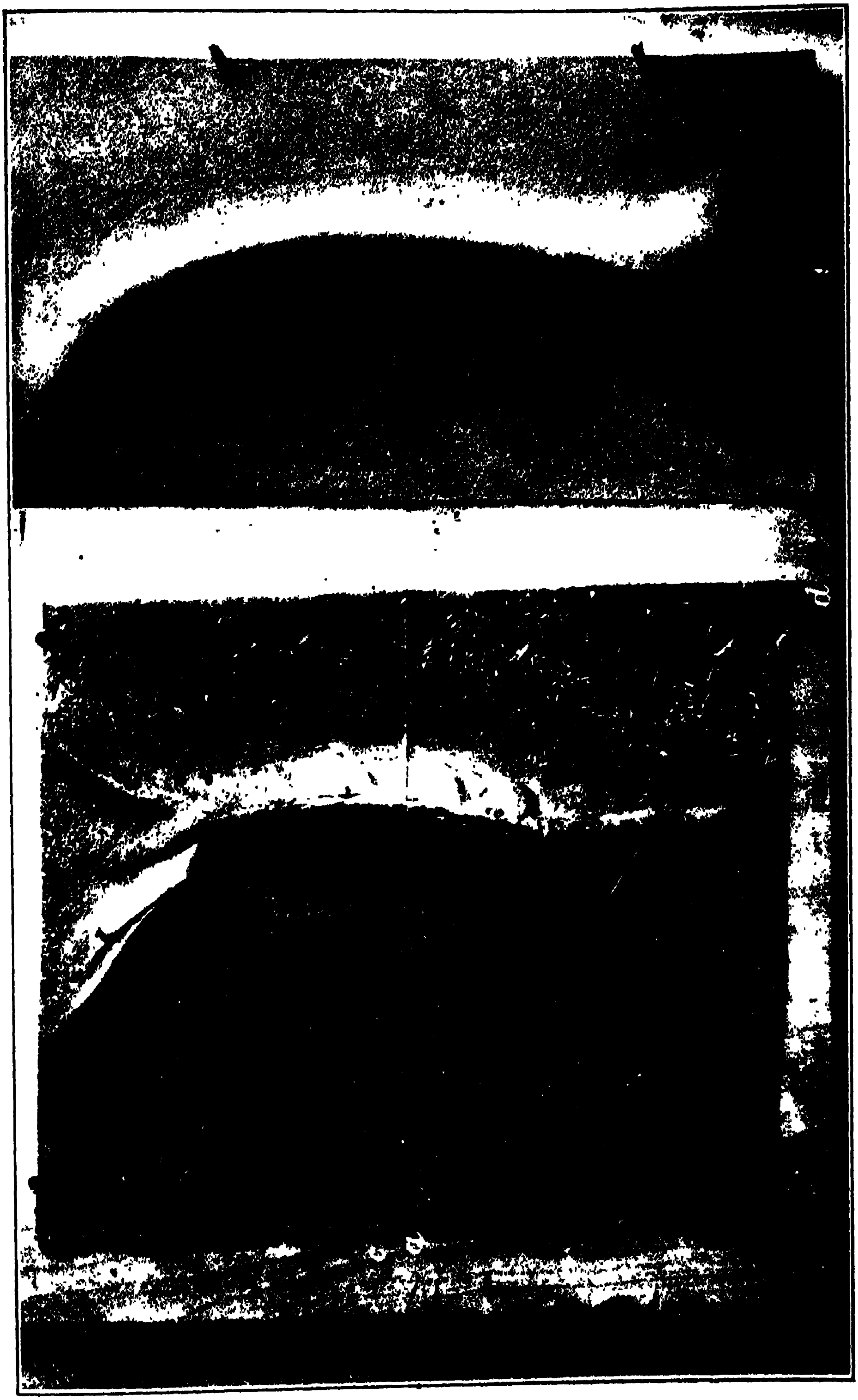


FIG. 2.

FIG. 1.

PLATE V.

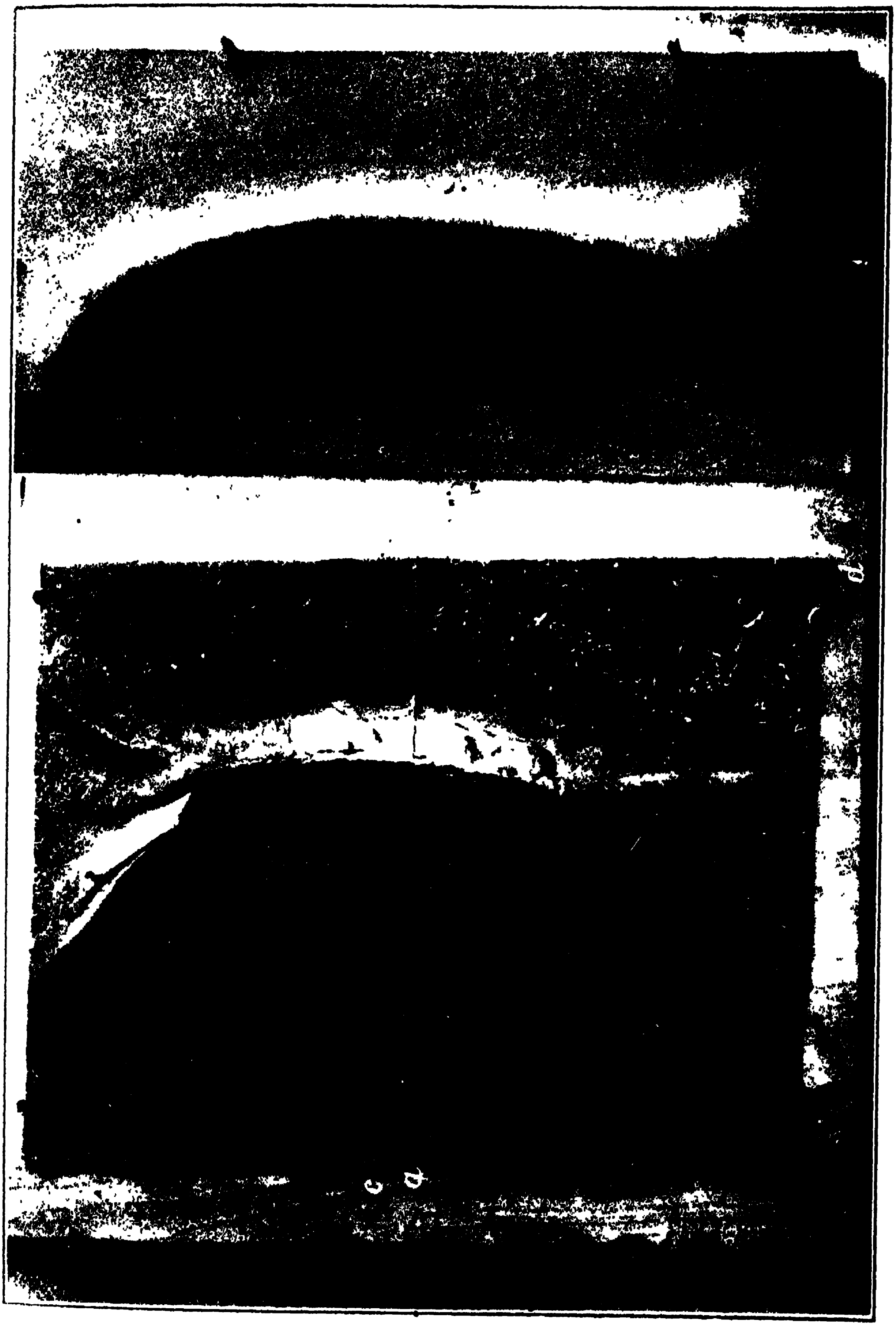


PLATE VI.

present external configuration. In nature, while the lateral pressure and ridging up were in process, sub-aërial denudation at a rate somewhat less rapid would have been cutting down and truncating the folds and ridges of our mountain range, and so exhibiting to us the wonderful structures the genesis of which we are investigating.

CHAPTER XI

EXPERIMENTAL MODELS

IN 'The Origin of Mountain Ranges,' pp. 331–33, I gave a description of certain experiments made by me in illustration of some of the principles expounded in that work. In consequence of these having been undertaken while the book was in the press the matter was necessarily compressed. This was in the year 1886. Since then more elaborate experiments have been made by Cadell on the same lines,[1] and later still by Bailey Willis.[2] Their apparatus and methods of compression were in principle the same as those employed by me and described in the pages referred to.

The objects aimed at were, however, somewhat different. Cadell was desirous of discovering in what way the huge thrust planes and intense folding of the Highland rocks were produced. Bailey Willis held the same object in view with regard to the Appalachians, but also aimed at elucidating general principles. My investigations

[1] 'Experimental Researches in Mountain Building' (*Trans. Royal Soc. of Edin.*, vol. xxxv. Part 7, 1888).

[2] 'The Mechanics of Appalachian Structure' (*Thirteenth Annual Report of the U. S. Geological Survey*, 1891 92; published 1894).

were undertaken partly to test some of the principles previously geometrically reasoned out in the body of the work.

I have thought it worth while in this further consideration of the subject to give photographs of some of the results arrived at in 1886. Being of clay, the contorted beds in the models have naturally shrunk after fifteen years' keeping in a dry cupboard, but not so as to vitiate the original description given in 'The Origin of Mountain Ranges.'

In addition, I give diagrams and photographs of experiments on composite bars made up of sheet-lead and paper and sheet-lead, millboard and paper. I also give some curious results of the compression of moist sand. These experiments were all made between December 1886 and February 1887.

I must here take the opportunity of remarking that, from the complexity of the subject, it is necessary in such experimental investigations to deal with one phase at a time of the movements the earth's crust is subject to.

Compression of Composite Bars.—The experiments detailed by Cadell and Willis were made upon composite bars of clay, plaster, and other materials of varying tenacity and plasticity, grouped in a trough and compressed from the ends. This produces vertical folds, accompanied with compression on the concave and tension on the convex side, the movements being in parallel vertical planes. To any one who has studied 'The Origin of

Mountain Ranges' it is hardly necessary to say that this is what seldom occurs in nature. Anticlinals, as there pointed out, are mostly ellipsoidal in shape, and are due to converging compressive forces; that is, to centripetal pressure.

Domical Structures.—In the experiment shown in fig. A, Plate XLII., 'Origin of Mountain Ranges,' I attempted to produce this effect of converging pressures, with partial success.

I now give photographs of the resultant model, which may be called a truncated anticlinal (Plate VII.). This shows its gradual development from an anticlinal of a small sectional area in the bottom bed to a larger one in the middle, increasing to a maximum at the top. It will be observed that the anticlinal is curved not only transversely, but also at right angles thereto, being a truncated ellipsoid, due to the pressure producing it having been to a small extent converging. This experiment is described in detail on page 163, Experiment No. 7.

Differential Expansion, the Cause of Folding and Domed Structures.—But it is by the expansion of lead plates by differential heating that the most instructive results have been obtained.

Such differential expansion produces a much more exact imitation of the forces producing the folding and compression that takes place in the building up of a mountain range. The forces are in the case of differentially heated plates *internal* to the affected area, whereas in the experiments by the authors named, and by others who have

PLATE VII.

Fig. 3.

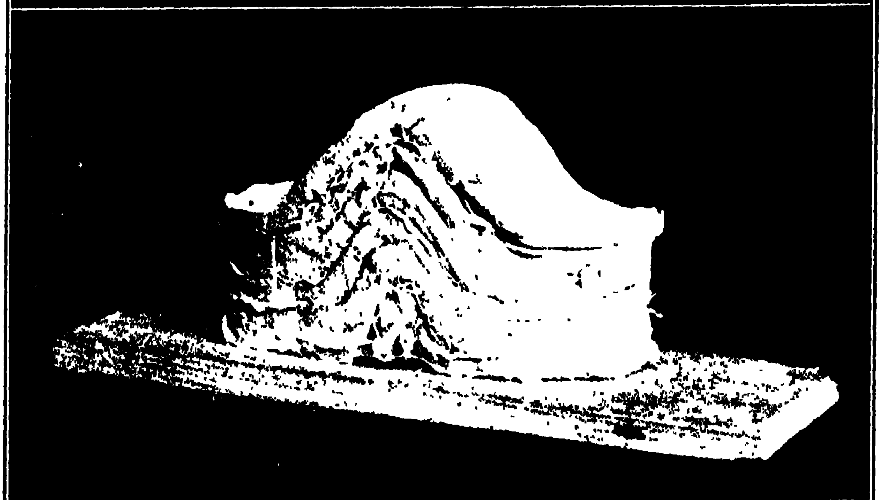


Fig. 2

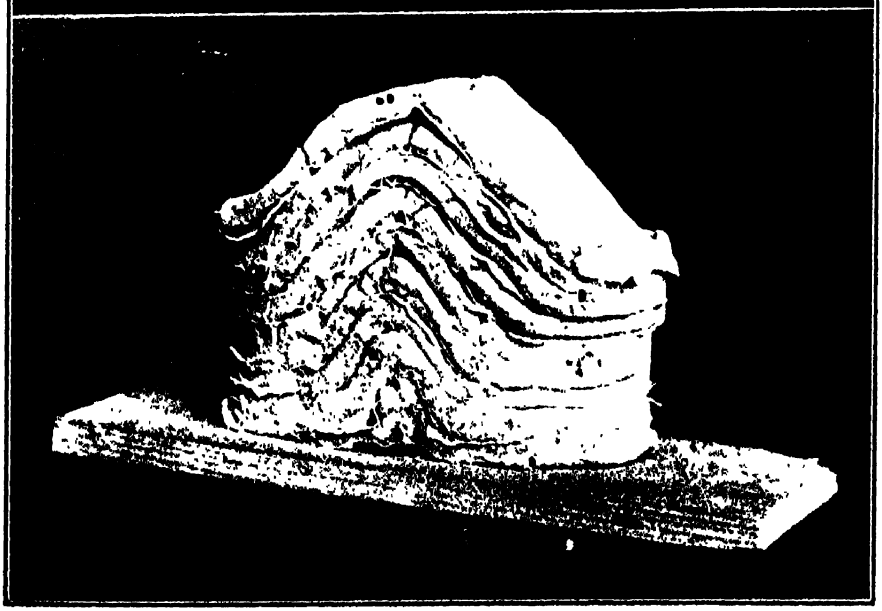


Fig. 1

attempted the investigation of analogous physical questions, the force applied has been *external.* Mountain ranges are manufactured out of extended sheets of strata, of a most composite nature, of very small thickness compared with their superficial extent. These compound sheets I have called strata-plates. By differential heating the outer parts of the area gradually shade off to the normal temperature. The consequence is that the folds come on in long, low undulations, increasing in amplitude till the centre of disturbance is reached. The mountain undulations grow out of the originally horizontal or nearly horizontal strata. There is a continuity between the initiatory and the central folding which cannot be imitated by forces applied to the exterior of the folded area. The anticlinals rise gradually out of the horizontal plane.

An examination of Plates III., IV., V., and VI. in the preceding chapters will illustrate my meaning better than any verbal description. The history of the lead bottom of the sink is the most instructive of all the examples of the effect of differential expansion on sheets of lead and, by parity of reasoning, on other material.

The anticlinal ridge as shown in Plate VI. of 'The Origin of Mountain Ranges' grew out of the lead plate by such infinitesimal movements as to be quite imperceptible, except by comparison at very long intervals. It would, I believe, be impossible by any amount of pressure applied to the

external sides of such a sheet to imitate this folding, which is due to differential internal lengthening and a flow of material from the centre of the plate towards the folds. Were outside pressure applied, the result would be that any folding which took place would entirely cross the sheet in one direction or another. Instead of imperceptibly growing out of the sheet, the fold or folds would traverse it to the sides and show on the outside edges as truncated anticlinals.

Anticlinal Dome.—These principles are illustrated in the simplest form in Plate II., fig. 4, 'Origin of Mountain Ranges,' where a dome-shaped circular anticlinal is shown which was bossed up by differential heating. The application of a gas-jet to one locality in a sheet of lead will boss it up at that place in a form answering to the form of the heated area.

If external compression were applied to such a circular sheet of lead, the effect would be that in the effort to reduce the area of the plate the exterior rim would become a series of folds. This can be shown by a very simple experiment, without any apparatus other than a circular piece of cloth, a board, and a few pins. Plate VIII., fig. 1, shows the result of the reduction of a circular piece of cloth 9 inches in diameter to 8 inches in diameter. By no other means than by folding at the edges can the area of the cloth be reduced, and the *quasi*-dome structure shown in the photograph is formed by the meeting in the centre of four anticlinal spurs, the

PLATE VIII.

Fig. 1.

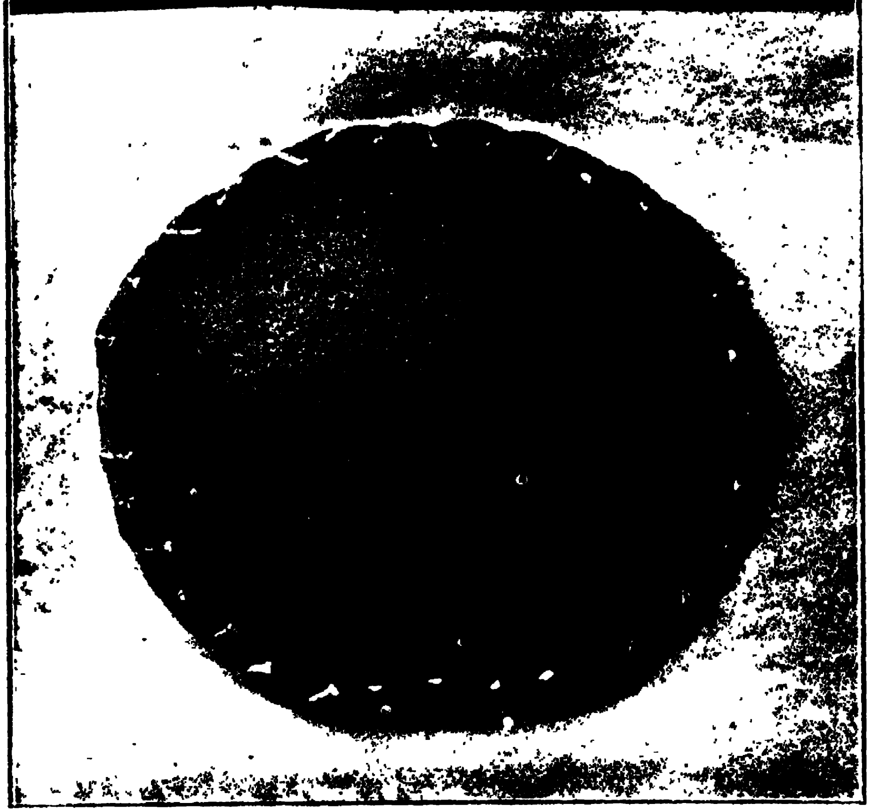


Fig. 2.

periphery being reduced by overlapping folds pinned to the board. See fuller details, p. 167, Experiment No. 10.

This, like many simple experiments, is more instructive than some on an elaborate scale involving much machinery.

It is illustrative of what may take place in the *focus* of maximum pressure of a compressed and disturbed area, *because as this focus is approached there is greater movement, and the forces originating within the strata-plate increase in intensity.*

Matter has Three Dimensions.—In the consideration of problems such as this the fact must never be lost sight of, however thin the plates may be, that we are dealing with matter having three dimensions, and in Nature's efforts to readjust the matter to new conditions a straining and flow of material from one *locus* to another must take place. The movements are extremely complex, but there is one principle that these experiments emphasises, namely, the *potency of time.* Put even the most rigid and intractable material under stress proportioned to the work required, and leave it sufficiently long under these conditions, the material by internal movements will adjust itself to such forces.

Though these experiments with the differential heating of lead plates, in my opinion, imitate more nearly the movements conditioning the formation of mountain ranges than do any other experimental methods known to me, there is one difference that

must not be omitted in our considerations. It is this. The rigidity of the earth's crust is small, considering the enormous forces which act upon it; consequently, excepting near to the surface, the material is in one way or another packed solid—no hollows will occur under the anticlinals.[1] Under such limitations as these, for a proper understanding of the problem some other form of experiment has to be resorted to.

The compression of less rigid material, such as layers of moistened clay, that tend to keep solid by their own weight, enables us to form a pretty accurate estimate of the form this packing would assume under conditions translated into the enormous scale of nature's operations.

Limit of Distance of Thrust conveyed through the Earth's Crust.—As bearing upon the possibility of stresses being conveyed long distances through the earth's crust by outside compression, I would direct attention to the difficulty experienced by every experimental investigator in subjecting even short bars to end compression. The anticlinal fold has a strong tendency to rise up near to the moving end of the compressor.[2]

[1] T. Nelson Dale, in Bulletin No. 195, Series C, *Systematic Geology*, U.S. Geological Survey, gives examples of 'Caves formed by tension and rupture' in the Ordovician schists of the Green Mountain region. These are attributed to movements later than the folding of the schists.

[2] See 'Mechanics of Appalachian Structure,' Bailey Willis, *Thirteenth Annual Report U.S. Geo. Survey*, 1891-92: 'After a number of experiments I began to be embarrassed to explain the constant

If the material of the compressed bar be made too rigid, it will not fold solid; if too plastic, it will not be stiff enough to convey the thrust. The inference is forced upon us that the strata of the earth, so far as affected by outside pressure, can only convey a thrust for a limited distance. The contraction theory, which attributes the folds in the earth to lateral pressure produced by a rigid crust following a shrinking nucleus, provides only *outside* pressure for the ridging up of mountain ranges. We may well ask whether this want of rigidity does not put an important limitation upon the extent of the areas which can be acted upon by such a cause. Experiments show conclusively that it is difficult to convey thrust through a plastic medium by end compression. How much more difficult does it become when the medium is not a mere bar, but an immense surface or plate, the movement, compression, and bending of which are necessary to build up mountain folds!

Pressure originating in Expansion Internal and Equable.—It is well here to draw a comparison between the effects of outside pressure and pressure originating in the expansion of a heated strata-plate. In the latter case the stresses originate in the plate itself, and are equally distributed throughout, proportionally to the differential heating. The most highly heated area is the *locus* of greatest

occurrence of an anticline at the end of the model nearest the piston' (p. 243).

movement. Every part of the materials of the affected area is under stress and strain, and the actual movement is produced by the enlarging of the superficial area of the strata-plate, which adapts itself to the new conditions by folding, doming up, or shearing, in a manner well exemplified in Plates III., IV., V., and VI.

PLATE IX.

Fig. 2.

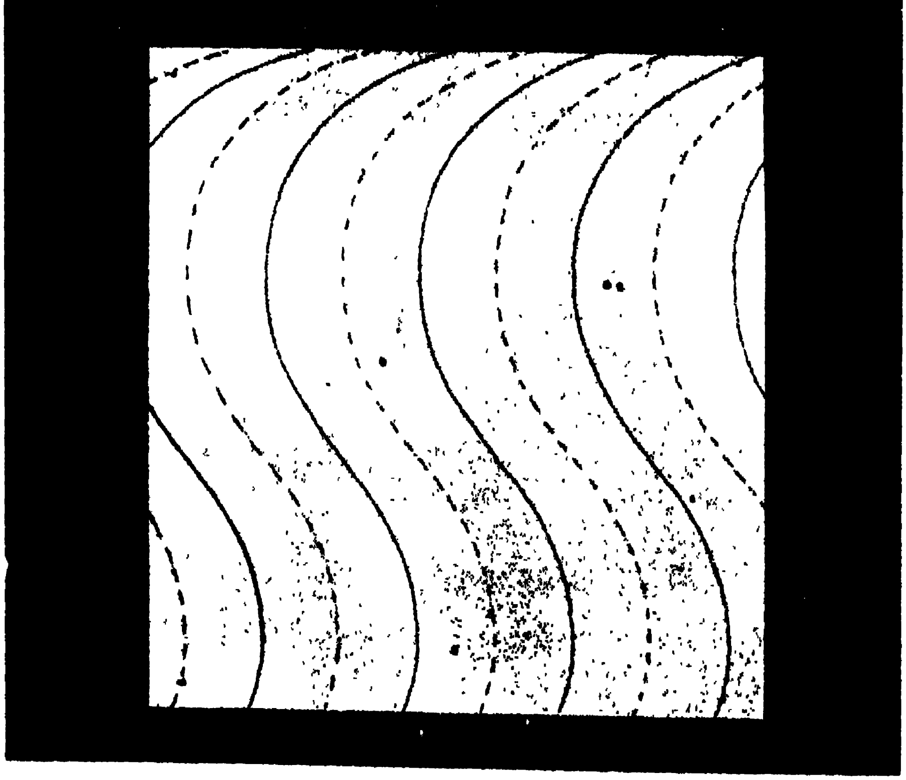


Fig. 1

CHAPTER XII

DEVELOPABLE SURFACES

DEVELOPABLE Surfaces.—I have said that in the analysis of these complex problems it is necessary to consider parts of the subject separately, to isolate them, and study the problems in their simplest aspects. I am indebted to my son, Mr. M. Treleaven Reade, for pointing out that certain geometrical forms can be developed out of plane surfaces without extension, compression, or stress or strain. These are called *developable surfaces.*[1]

The application of this principle to the study of mountain structure is of interest in enabling us to elucidate some of the complexities of the subject. Mr. M. Treleaven Reade has been good enough to make the model shown on Plate IX., figs. 1 and 2.

[1] See 'Some Properties of Flexible Surfaces and Flexible Solids' (*Trans. of Liverpool Engineering Society*, vol. xx. Session 1898–99). A superficially rigid surface which can be unrolled so as to lie wholly in one plane is known mathematically as a '*developable*,' and it is only this class of surfaces and their combination in various stages of development that the author treats of in this paper. The 'developable' surfaces consist of the cylinder, cone and torse. They are a distinct geometrical conception from the truly mathematical surface, in that they are assumed to possess the material qualities of unstretchability and unshrinkability, or, as defined before in other words, they are 'superficially rigid.' For all practical purposes we may regard 'developables' as sheets of material thin enough to bend easily, but thick enough to be 'superficially rigid' (p. 191).

Fig. 1 shows a rectangular surface, formed out of drawing-paper, measuring 6 inches by 6 inches. The curved lines drawn upon it represent scorings made by a knife alternately on opposite sides of the paper, the firm lines representing the ridges, the dotted lines the valleys, as developed in fig. 2.

Fig. 2 shows this surface folded into curved symmetrical forms: a symmetrical ridge-and-furrow system of mountains. The surface area of the folded system is exactly that of the unfolded parallelogram, or 36 square inches. The basal area of the folded system measures 24·12 inches, or 11·88 square inches less than when flattened out as shown by the encircling white line.[1] It will be observed that the basal plan of the folded system assumes an irregular form, and that in the development of the ridge-and-furrow folds the horn of the fold at *a* has moved in a spiral.

Examined analytically, it will be found that, excepting what is due to the thickness of the paper, there is, theoretically, neither stress nor strain involved in the folding; and this is true of all the forms of developable surfaces. Translated into the concrete, it will also be observed that to fold up a system of mountains of this form and character compressile movements would have to be applied in variable degrees to the exterior of the parallelogram.

[1] These areas have all been measured with the planimeter.

PLATE X.

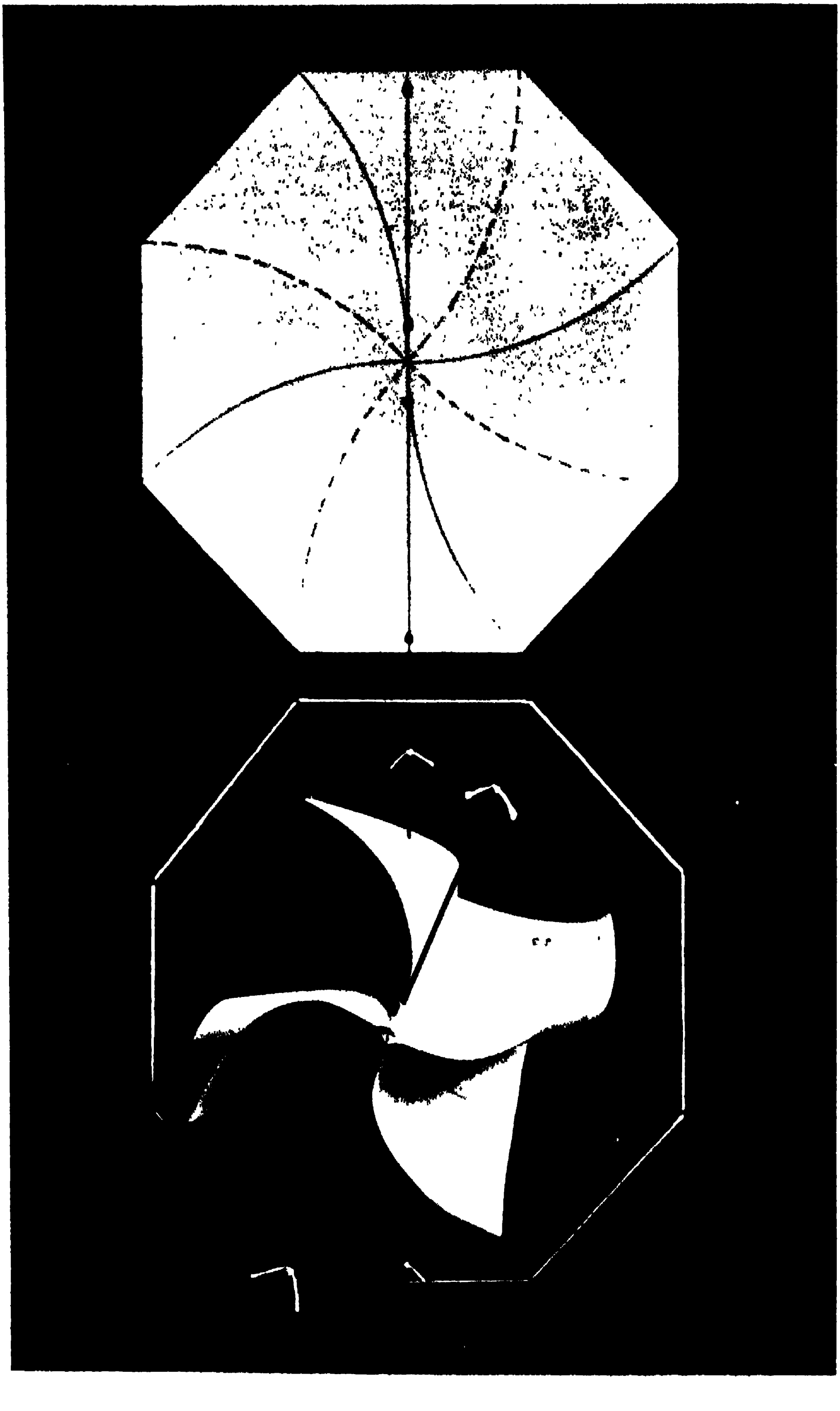


FIG. 1.

FIG. 2.

Plate X., Fig. 1.—This represents an octagon formed out of similar paper to fig. 1, Plate IX. It is 6 inches across and scored with a knife alternately on opposite sides, but in this instance with radial curves. The area by measurement is 29·73 square inches. Fig. 2 represents the same sheet in the folded form, the basal area being 10·98 square inches, or 18·79 less than the original octagon. The folded surface area is precisely the same as fig. 1, and is shown by the enclosing white line.

The interesting feature of this experiment is that in the folding the central area takes on a spiral movement. To show this effectively a pointer has been pinned to the central part. On comparison of this with the fixed pointer, it will be seen that it has moved round through 26°.

It must not be thought that Nature ever exactly reproduces the conditions of any of the experiments by which we have been trying to diagnose earth movements and get a grip of the conditioning factors.

The last experiment bears upon the possibility of strata in compression developing torsion structure such as Mrs. Ogilvie Gordon has investigated in the Dolomites.[1]

In what Professor Lapworth aptly calls tri-

[1] *Q. J. G. S.*, vol. lv. pp. 560 634. See also report of meeting of the Geo. Soc. of Edinburgh (*Nature*, February 12, 1903), where Dr. Gordon gave her views of 'Simultaneous Duplex Crust Movements in the Fassa-Monzoni District.'

dimensional movements it seems as if anything were possible, even 'mushroom-shaped mountains without roots.'

Further experiments will be detailed bearing upon this interesting problem.

CHAPTER XIII

DETAILS OF EXPERIMENTAL INVESTIGATIONS

The Folding of Compound Bars subject to End Compression

EXPERIMENT No. 1.—This was made with a series consisting of eight plates of lead, weighing 7 lb. to the superficial foot, measuring 1 ft. $8\frac{3}{4}$ in. long by 2 in. wide. Between each pair two layers of calico were placed. The combination A, fig. 1, Plate XI., was then placed on a 'sole' board, *a*, 2 ft. 5 in. long, having a side secured thereto for guiding the combination. Two pieces of board, *b*, *b*, were placed at each end, on top of the lead series, with a bridging board, *c*, covering all and spanning the space between. These arrangements will be easily understood on referring to Plate XI. The combined series was secured together and to the 'sole-board' by clamps at *d*, *d*.

The object of the space *e* was to allow the combined bar to rise in an anticlinal at that portion of the bar.

By means of a powerful cramp pressure was applied to the ends of the lead combination bar, which slid between the cheeks by which it was

confined. At first it took a considerable force to turn the screw of the cramp, and the lower plates at *f*, fig. 2, became contorted.

Continuing to turn the screw, the crown of the anticlinal (*h*, fig. 2) rose to $2\frac{5}{8}$ in. above the sole-board at *g*, the lead bar being shortened to 1 ft. $8\frac{5}{16}$ in. When shortened to 1 ft. $7\frac{3}{4}$ in. the height was $3\frac{5}{16}$ in.; when 1 ft. $7\frac{5}{16}$ in. the height was $3\frac{3}{4}$ in., and, when 1 ft. 7 in., 4 in., as shown in fig. 3.

The rise of the crown of the anticlinal, which brought it in contact with the bridging board, so bent the latter upwards that at this point it cracked at *i*, fig. 3.

The bridging board was now removed, and the screwing up continued until an overturn-anticlinal was formed, as shown in fig. 4, which measured $8\frac{1}{4}$ in. high and $9\frac{3}{4}$ in. at base, or a reduction of 11 in. in length of the base.

The pressure of the bridging board did not seem to appreciably influence the form of the anticlinal, and serves to show how great must be the lifting effect in nature of a rising and developing anticlinal upon the covering rocks.

The photograph, Plate XII., fig. 1, shows the completed overturn-anticlinal in perspective, from which a better idea of it as a solid can be obtained than from the diagrams.

NOTE.—I have not thought it necessary in figs. 2, 3, and 4 to draw in the individual plates, but have treated the combination as a bar. The photo

Fig. 1.

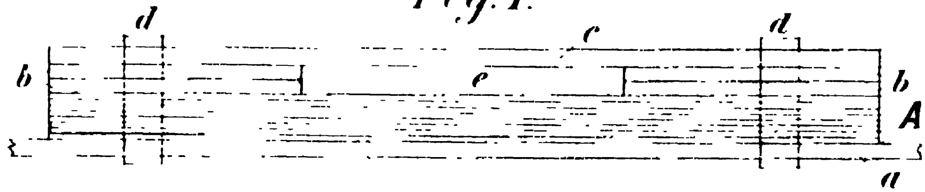


Fig. 2.

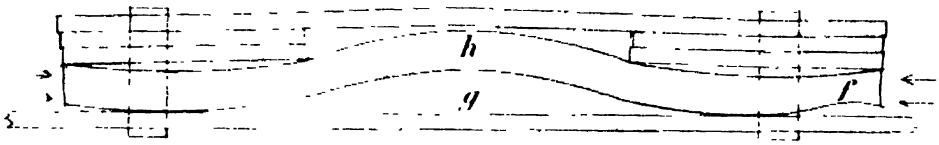


Fig. 3.

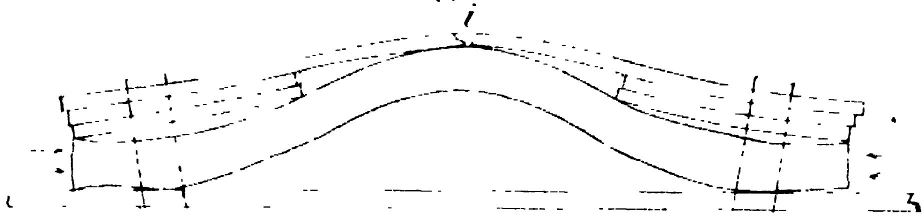
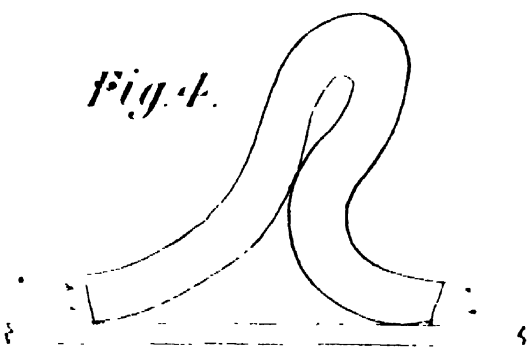
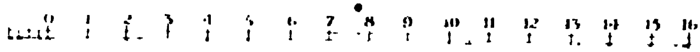


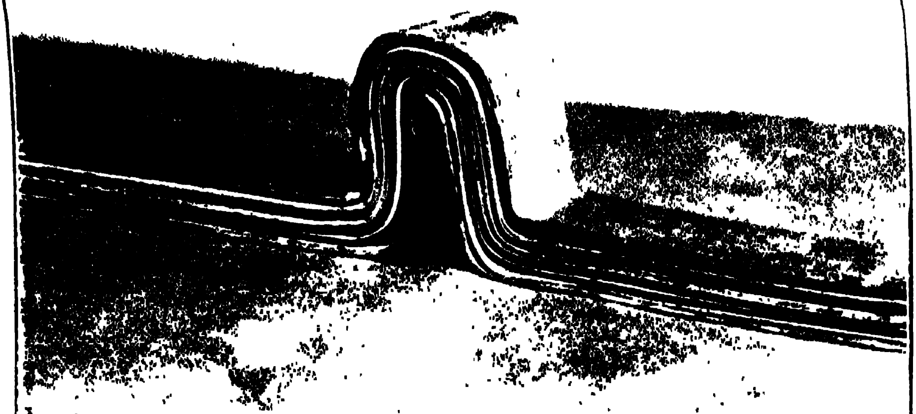
Fig. 4.





Scale of Inches

PLATE XII.



Fi

Fig. 1

shows the lead plates of which the bar is composed.

Experiment No. 2. (Plate XII., fig. 2.)—A compound series was prepared, 1 ft. 5½ in. long, consisting of three sheets of lead, three sheets of millboard, and five layers of paper, forming a bar 2 in. by ¾ in. in section. This combination was put into the apparatus just described, excepting that the bridging piece was sawn through the middle and the clamps were nearer together.

On applying pressure to the ends of the bar as before the upper layers rose rapidly in anticlinal form, the bottom sheet of lead at first remaining flat; it then suddenly sprang up. The pressure on the anticlinal could be regulated by the clamp screws and the elasticity of the boards they acted upon. Continuing the screwing up, the left limb of the anticlinal became vertical, the right being at an angle of over 40°. After this, by the more rapid approach of the bottom part of the left limb towards the right limb, the anticlinal closed up and became an overturn to the left.

The clamp screws were then removed and the screwing up continued, when a second anticlinal was initiated to the left, the various layers of the bars separating, and rising one inside the other in anticlinal form.[1] The right-hand end of the bar was unaffected.

Experiment No. 3.—The whole series was straightened out and placed anew in the machine.

[1] The photo does not show this.

Again the left-hand end moved most on screwing up, but this time the anticlinal became an overturn to the right.

These experiments seem to show that very minor causes may influence the direction of the movement of rocks subjected to great pressure.

Experiment No. 4.—A bar of soap, 1 ft. $2\frac{3}{4}$ in. × $2\frac{1}{8}$ in. × $2\frac{1}{2}$ in., was cut into two layers as shown in section fig. 1, Plate XIII., and then placed in the machine. The effect was to shear the ends sideways with a vertical shear (fig. 2). By no amount of coaxing, clamping, or regulation of the screw could I make this curious material act otherwise. The pressure and movement produced slickensided surfaces on the planes of shearing.

Experiment No. 5.—In this experiment a wooden trough was filled with alternate layers of moistened quartzose sand, and sand and coal dust mixed. This formed a bar 2 ft. $0\frac{1}{2}$ in. × 2 in. × 2 in. before compression. On applying compression the ends commenced to lift, so I placed weights upon them to keep the sand down. The effect was curious. The sand flowed from under and rose round the weights, and the bar of sand sheared beyond as at *a*, fig. 3, Plate XIII.[1]

After removing the distorted ends the bar measured $12\frac{1}{2}$ in. long; this was compressed to 11 in., but again the ends sheared in the way shown in fig. 4, Plate XIII. When the sheared ends were removed, the shear in cross-section

[1] Only one end of the bar is shown.

proved to be a portion of a large circle, this doubtless being caused by the friction against the sides of the trough and the freer movement of the central part of the sand.

The sand was just damp enough to hold together by surface tension, and the sharpness of the edges of the sheared ends (*a*, *a*, fig. 4) was very remarkable.

Experiment No. 6.—In this experiment the trough was filled with layers of moist sand, divided by three sheets of tea lead, and resting upon a bottom sheet of tea lead, making a bar 17¾ in. long and 2 in. square in section.

On screwing up the top layer of dark sand[1] began to rise at the end in an anticlinal with a cavity under. I placed a short weighted board upon it, which had the effect of keeping the layers together. The formation of overfolds resulted in the attempt of the sand to shear, which was prevented by the lead sheets. Fig. 6, Plate XIII., fully explains the resultant forms, and shows graphically how combinations of alternate soft and tenacious beds of rock may be affected by lateral pressure. The sand, having rounded grains and little coherence, flowed, and filled the spaces between the anticlinals and synclinals into which the lead was folded in adapting itself to the complex compressive and tensional forces.

Experiment No. 7.—Plate VII., p. 148, is a

[1] The ruled lines are only shading to show the layers darkened by coal dust; they do not represent the lead sheets.

series of photographs of a model resulting from the compression of an aggregate of nine layers of clay. This is described in 'The Origin of Mountain Ranges,' page 332, and figured in Plate XLII., fig. 4, of the same work; fig. 1, Plate VII., represents the complete series of nine layers as finally folded. Fig. 2 shows it with the three top layers removed. Fig. 3 shows the three bottom layers.

This is a good example of the way an anticlinal fold is developed.

It begins with a small, highly compressed fold at the base, and layer by layer gradually increases in amplitude, but becomes less compressed as the apex is approached. The apex layers are finally ruptured by tension.

By the insertion of two zinc horseshoe bands in the trough, bearing against the ends of the series of layers, the pressure was made slightly convergent, with the result that the anticlinal took the shape of an ellipsoidal dome truncated at each end. This form, together with double layers of thin calico between, separating the clay strata, enabled the layers to be taken off one by one, thus exhibiting the interior structure and mould of the anticlinal.[1]

[1] The following is the general description given in 'The Origin of Mountain Ranges,' figs. 4, 5, 6: 'Experimental illustrations of the movements of bedded rocks in folding by various applications of lateral pressure.'

The apparatus consisted of a wooden trough open at the ends, into which were fitted two sliding compressing bands, each formed of a strip of zinc bent round in horseshoe form and overlapping each other in the middle. The compression chamber, or contortion box, was then

EXPLANATION OF PLATE XIII.

FIGS. 1 and 2 represent a bar [illegible] subjected to [illegible] compression. The bar w[illegible] [illegible] shown by the middle horizontal line in the cross section [illegible] 1.

FIG. 2 is [illegible] of the bar [illegible] the [illegible] of the compressing forces. The result of the compress[illegible] was the establishment of a vertical shear [illegible] (Experiment No. 4).

FIGS. 3 and 4 represent experiment[illegible] with a bar of [illegible]. The dark strata [illegible] layers of [illegible] mixed [illegible] to help [illegible] the movements. The result wa[illegible] in the experiment [illegible] the [illegible] and from [illegible] the weight [illegible] was placed on the end [illegible] and perfection of the [illegible] and [illegible] held together by the [illegible] Experiment No. 5.

FIGS. 5 and 6 represent [illegible] by three layers of [illegible] lead and [illegible] the bar before compression. [illegible] 6 [illegible] and convolutions are the result of the [illegible] lead [illegible] shear as in Experiment No. 5 [illegible] bottom sheet of [illegible] lead (Experiment [illegible]).

For further explanation [illegible] the accompanying [illegible].

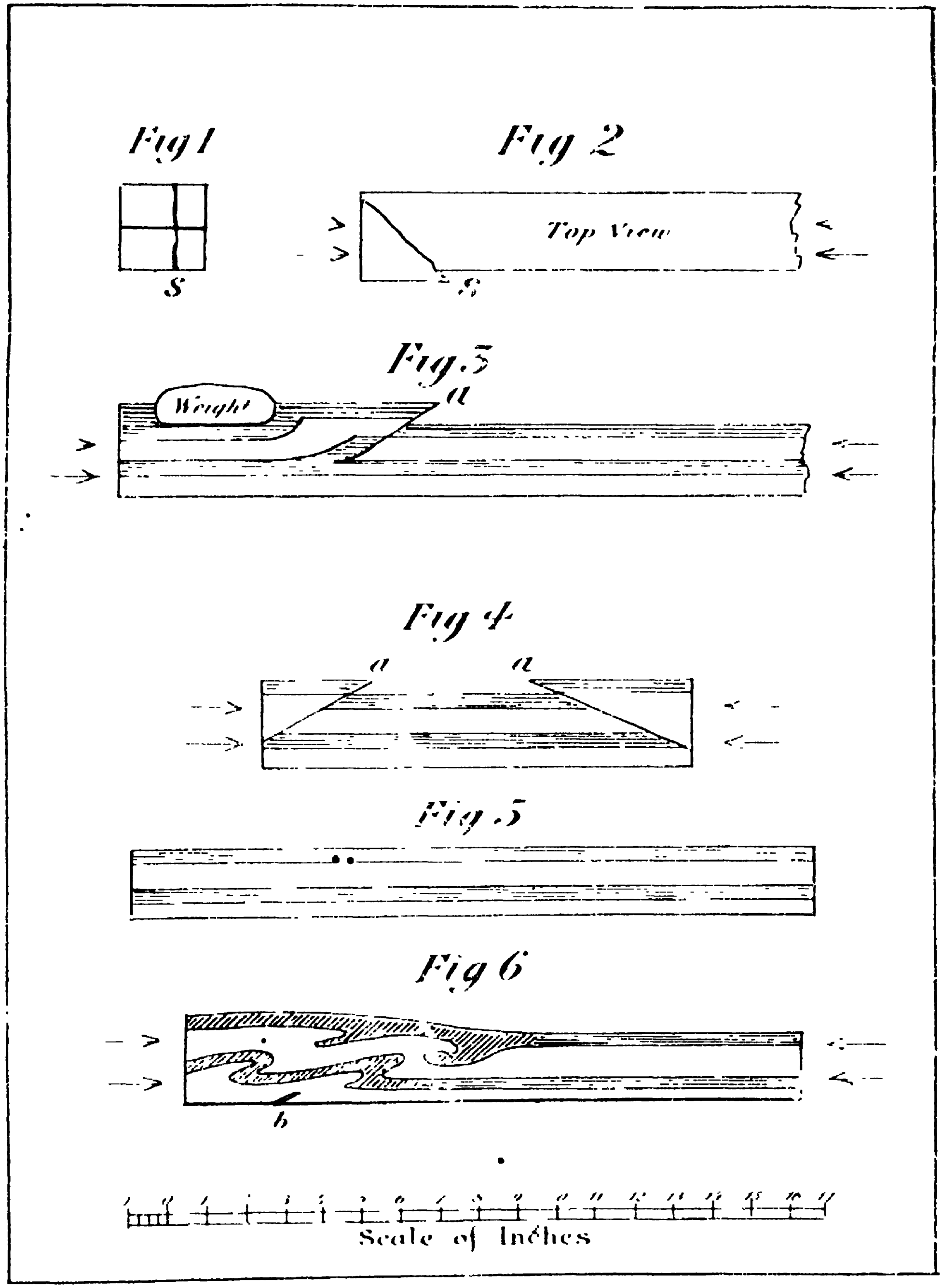


Fig 1
S
Fig 2
Top View
S
Fig. 3
Weight
a
Fig 4
a
a
Fig 5
Fig 6
b
Scale of Inches

Experiment No. 8.—Fig. 1, Plate XIV., is a photograph of an anticlinal built up of seven layers of clay with calico partings (as in preceding experiments), compressed from the ends, which were square and vertical, giving thereby parallel or non-convergent pressure, and consequently no domed development ensued. It will be observed that the building of the anticlinal took place near to the moving end of the compressor.

Experiment No. 9.—Fig. 2, Plate XIV., is a photograph of the anticlinal represented in diagram, fig. 5, Plate XLII., 'Origin of Mountain Ranges,' but showing the opposite side.

In this case the ends of the compressor were formed in step fashion, so that the pressure should first affect the basal layers and successively the superincumbent layers.

This model is to represent what I maintain actually takes place in the earth where the movements of the earth's crust are greatest at considerable depths, tailing off to nothing at the surface. It will be observed that the strata of the anticlinal are more compressed than in the preceding experiment and the tension at the apex greater.[1]

filled with the material to be acted upon, which in figs. 4, 5, 6 consisted of layers of a fine tenacious clay. The pressure was applied with a cramp against blocks of wood sliding in the wooden trough and acting on the zinc bands. The layers of clay were free to move vertically. In correction I may add that the zinc bands were only used in model fig. 4; in 5 and 6 stepped blocks of wood were used, as is, indeed, described in the details of each experiment.

The reader who wishes for further particulars should refer to the original work.

[1] A paper by F. A. Steart, Esq., upon 'Overthrusts and other

Fig. 3, Plate XIV., is a photograph of the contorted beds figured in 'The Origin of Mountain Ranges,' fig. 6, Plate XLII., but exhibiting the opposite side. The object of this model is to illustrate what would take place if the surface layers of the earth were compressed the most, and the pressure decreased to zero at a certain depth below the surface, which would happen on the contraction theory. The result has been attained by reversing the stepped end of the compressor, so that the pressure came first upon the top layers. In neither of the last two examples were there any calico partings between the layers.

Disturbances in the Braysdown Colliery,' Somerset (*Q. J. G. S.*, vol. lviii. pp. 609–17) describes phenomena bearing very closely upon this experiment. Considerable overthrust faults were disclosed by the workings, similar in character to those at Radstock, but of less overlap and throw. Mr. Steart considers both sets to be part of the same movements. The interesting fact recorded is that the overlap increases rapidly with the depth. *The bottom beds are evidently longer than the top beds.* This is a feature to be expected on the principles enunciated in this work and 'The Origin of Mountain Ranges' and illustrated in Experiment No. 66 (p. 153). A rise of temperature proceeding from below upwards would expand the bottom beds first and to the greatest extent, other things being the same. On the other hand, the beds of different kinds of rock have varying coefficients of expansion, and this, again, on a change of temperature will cause differential movements. Mr. Steart also proves that the soft black shales have got stripped off in one place and piled up in another, making up what the miners call 'dead ground.' Careful detailed observations such as these are invaluable.

PLATE XIV.

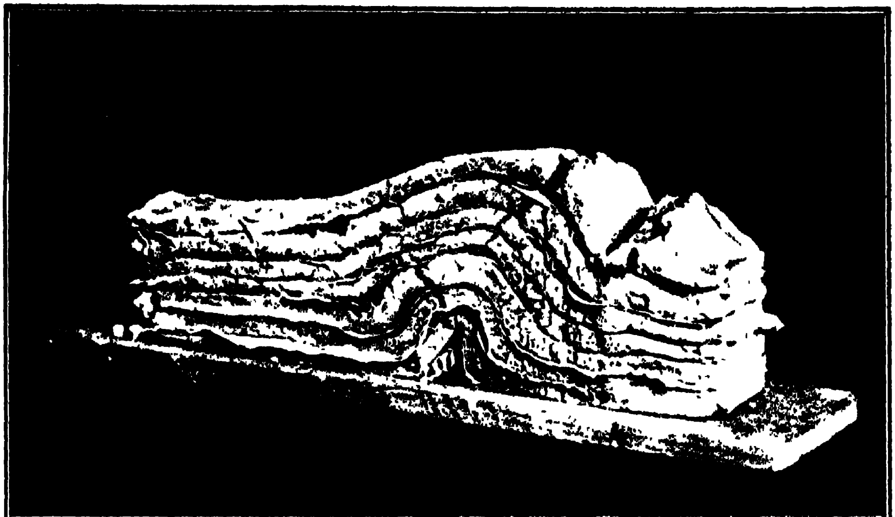


FIG. 1.

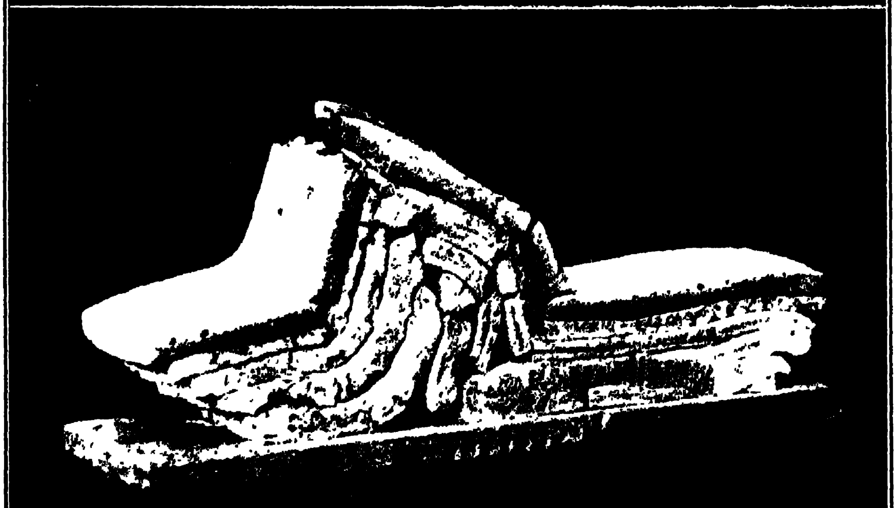


FIG. 2.

FIG. 3.

CHAPTER XIV

EXPERIMENTS IN THE COMPRESSION OF SHEETS OF DIFFERENT SUBSTANCES BY CONVERGING PRESSURE APPLIED AT THE EDGES

THE FORMATION OF DOMED ANTICLINALS

EXPERIMENT No. 10.—Preliminary to the actual compression of sheets into domed structures, with the object of elucidating the movements and flow of material that must take place in their formation, I took a piece of apron linen and cut out a circle 9 inches in diameter. The problem I set myself was to reduce the periphery to 8 inches diameter. With material such as cloth the only possible mode of reducing the circle is by folding at the edges, the tendency being to force up a quasi-dome formed of radial anticlines. An examination of Plate VIII., fig. 1, p. 150, will show that in this case I reduced the circular area covered by the linen cloth from 9 inches to 8 inches diameter by folding at the periphery in four places, the folds being pinned down to the board. This reduced the area covered by the cloth from 81 circular inches to 64 circular inches, which formed the area of the base of the intended dome. These folds, being all in one direction, created a

series of four radial curved anticlinals, meeting near the centre, and forming there an irregular domical centre which measured about 2 inches in vertical height.

The illustration explains itself and shows the form a non-rigid sheet will take in adapting itself to the new conditions of space. It is this sort of folding by which drapery adapts itself to the movements of the human body in an infinite variety of ways, to the delight of the sculptor, who rejoices in the beauty of form which it discloses though it clothes.

Experiment No. 11.—The next material experimented with was what is called 'cork carpet.'

It is a pliable material, possessing a limited compressibility, and also capable of a little stretching without rupture. The thickness of the material was in this case a full quarter of an inch.

Out of this Mr. M. Treleaven Reade cut a circle 5 inches in diameter, which was planted upon a board. Wire nails were driven into the board just touching the periphery of the circle, and then bent down so as to claw the periphery down to the board. The circle was compressed by a screw-cramp, and a second ring of nails driven in, the cramp being moved round the circle as the operation went on, the nails being clawed down as before. The photograph, Plate VIII., fig. 2, p. 150, shows this.

The advantage of this process, besides its simplicity, is that the model retains its form, which

it would not do if a compressing machine without holding-down nails were used.

The result was the arching up of a very perfect dome having a diameter at the base of 4·62 inches, or a reduction of area from 25 circular inches to 21·39 inches. The height of the dome was ·87 inch.

This proved a very interesting experiment; it showed that the tendency to fold at the periphery being prevented by the clawing down of the nails was compensated by compression of the peripheral area. I may add that the resultant dome was immensely strong.

Experiment No. 12.—We next took a similar disc, also 5 inches diameter, and by the same process compressed it into an ellipsoidal form, the longer axis of the base being 5 inches and the shorter $4\frac{1}{2}$ inches. The result was a perfect arch along the longitudinal section rising $\frac{5}{8}$ inch vertically. The length of the arch was $5\frac{1}{4}$ inches. The section along the short axis commenced with a concave curvature from the periphery towards the centre, which changed into a prominent convex curve as it approached the centre.[1]

An analysis of the movements proves that the material *stretched* along the long axis and was *compressed* along the short axis; probably an area of the apex of the dome was stretched in both directions.

[1] I have thought it unnecessary to reproduce this photo.

CHAPTER XV

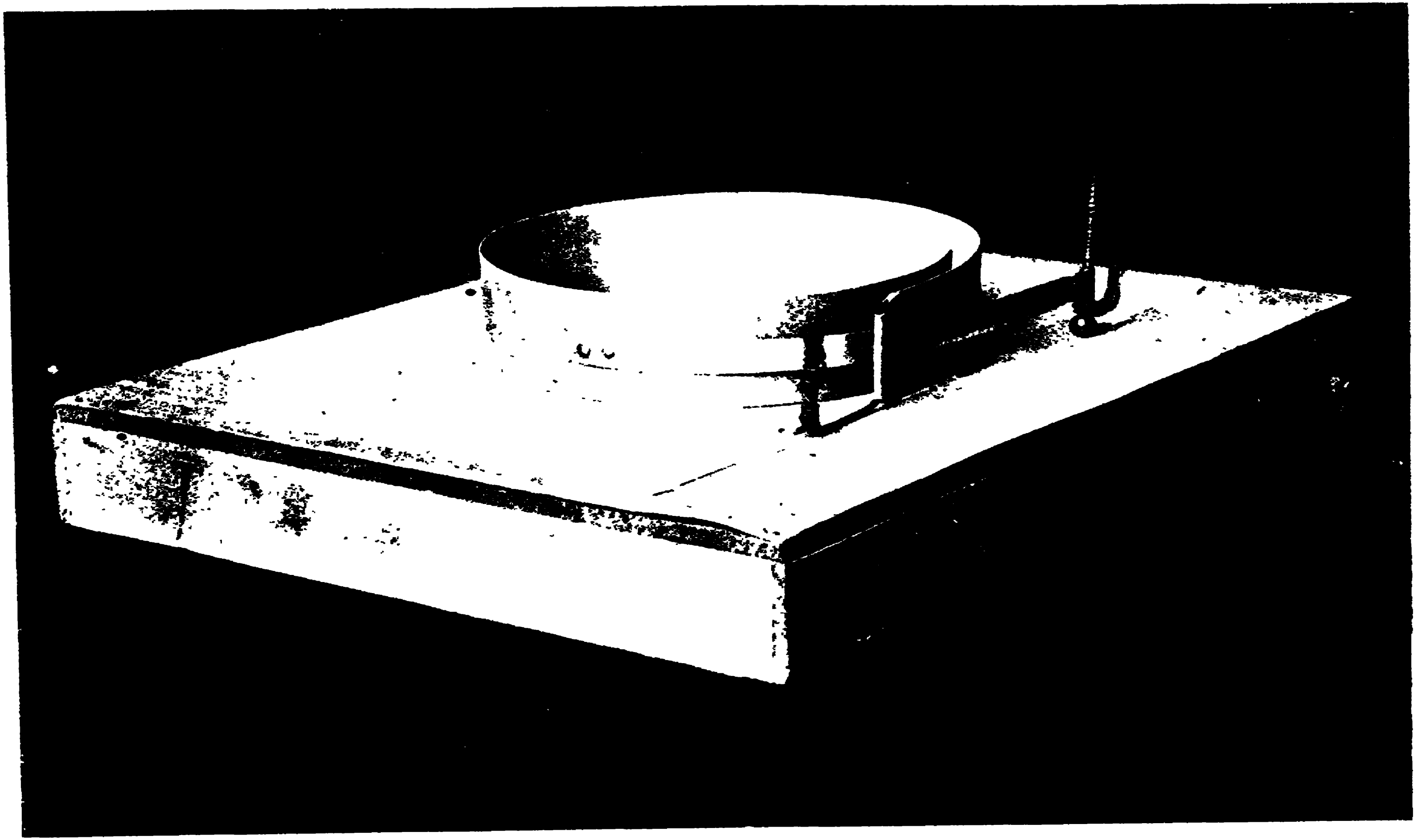
FOLDING OF CLAY STRATA-PLATES BY CIRCUMFERENTIAL COMPRESSION

THE Compressor.—With the object of applying circumferential compression to circular discs or plates of clay a 'compression band' was devised.

This consisted of a thin pliable band of zinc, 2 inches wide, to which a strap of similar zinc $\frac{9}{16}$ inch wide was riveted at one end to the 2-inch band, the other end being passed through an aperture or slit near the opposite end of the 2-inch band. A handle was fixed to the free end of the strap, and another to one end of the band. When these handles were drawn apart, in the direction shown by arrows on the photo, Plate XV., by means of a cramp-screw, a gradual tightening of the band took place and a reduction of the circumferential girth. The photograph of the actual machine (Plate XV.) will fully explain the extremely simple action of the 'band compressor,' which was made for me by my son, who also assisted to carry out the following experiments.

The material used in the experiments was modelling clay. This was made very plastic and

PLATE XV.



rolled into plates of the required thickness. The discs were cut out to a circular template of $9\frac{5}{8}$ inches diameter. To prevent the clay sticking to the metal plate on which it was rolled, the plate was smeared with sweet oil. The inside of the band-compressor was oiled for the like purpose and to diminish friction; as also the bed-plate on which the clay disc was placed to be operated upon, care being taken that the oil did not get mixed with the clay during preparation, as otherwise the lumps of clay on being rolled out would not adhere and combine.

When the discs formed a combination or series, they were in the first experiments separated by two layers of tissue paper. It was found after trial that fine sand was better for this purpose. By this means the separate layers or plates could be taken apart and replaced at pleasure, excepting in those cases where there occurred an overfold which interlocked the plates.

Experiment No. 13 (Plate XVI.): *Domed Folds.* Three sheets or discs of clay, $\frac{1}{2}$ inch thick each and $9\frac{5}{8}$ inches diameter, were superposed upon each other, separated by tissue paper, as already described, to prevent adherence, the whole compound being placed upon the oiled bed-plate.

The compressor was adjusted to the circumference of the compound disc and gradually tightened. After a certain amount of screwing up a domical thickening began to develop near to the part of the band between the handles, and

rose nearly to the top of the belt. A smaller domical anticline began to appear at the opposite side of the band. After the first had risen above the top of the band, the band was slackened and turned round, so that the second anticlinal took the place of the first. This then began to develop rapidly, and when eventually screwed up so that the circle was diminished to $8\frac{1}{4}$ inches diameter the model reached its present form, as exhibited in Plate XVI. and in explanatory diagram, Plate XVII., fig. 1.

A total thickening of the three layers of about $\frac{1}{4}$ inch took place, in addition to the domical uprise. On taking off the band it was seen that the larger dome was a truncated fold. The lesser dome was also in a minor degree truncated.

The underside of the bottom layers was concave beneath the domes. A wrinkling and shearing of the upper surface of the clay became noticeable on drying.

The compression caused a reduction in area of the circle, as shown in fig. 1, Plate XVII.

Description of Plate XVI. —Fig. 1 shows the compressed compound plates complete, as they left the machine, photographed after drying. The fold to the left is the larger dome, that to the right the smaller.

Fig. 2. The top plate has been removed and the upper surface of the middle plate exposed. The domical uprise is sharper than in fig. 1 and more fissured.

PLATE XVI.

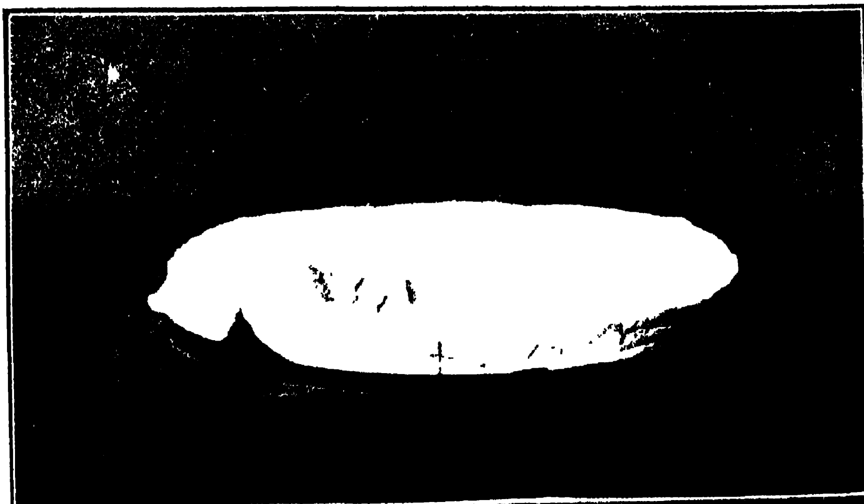


Fig. 3

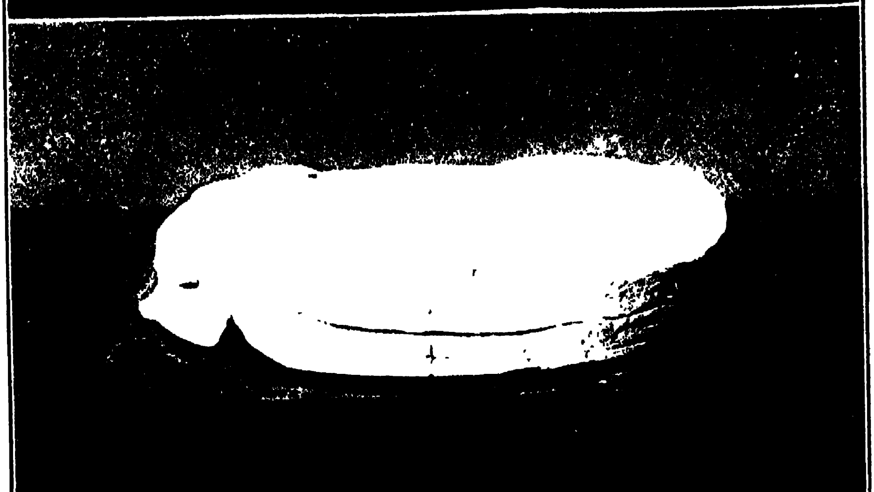


Fig. 2

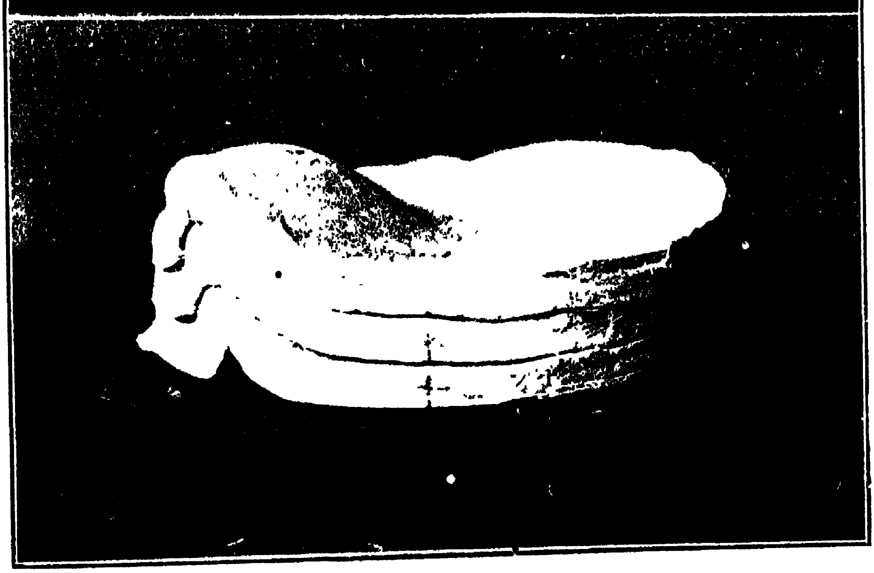


Fig. 1

Fig. 3. This shows the bottom plate only, the two above being removed. The surface of the larger dome is very much crumpled and sheared, as can be seen in the view.

Fig. 1, Plate XVII., shows the areal reduction of the assemblage of discs, the position of the domes, and the direction of movement of the clay.

Experiment No. 14 (Plate XVIII.): *A Plagioclinal Mountain.*—A bottom bed of clay was in this case prepared with a low elevation modelled upon it, representing what might be an undulation of the surface having its axis along a diameter of the circle. This was cut with a knife into beds with a high dip stretching obliquely across the axis. Fine sand and charcoal was dusted between the beds. This base bed was intended to represent a complex structure analogous to the *Uriconian* rocks, upon which the Cambrian rocks of the Wrekin were laid down, and which now forms the core of the mountain. Upon this undulating surface four thin layers of clay, combinedly averaging 1 inch thick, were moulded and pressed, each layer being dusted with the black sand to prevent adherence. The whole assemblage of associated beds was now put into the compressor, and a general domical anticlinal began to rise. The material evinced a tendency to thicken and turn up at the edges towards the tightening bands, and to modify this the compressor band was slackened and the compound moved round a little occasionally.

The base was reduced from $9\frac{5}{8}$ inches diameter

to 8 inches across in one direction and 8¼ inches in another, the circle being to this extent distorted. The domed uprise was in position partly determined by the modelled axis of the base bed.

The base bed developed a very pronounced ellipsoidal anticlinal dome having a hollow on the underside.

This is remarkable, as the beds being cut by the knife were separable pieces (see fig. 3, Plate XVIII.).

The compression acting upon the base bed produced on it an uneven upper surface (see fig. 3, Plate XVIII.). The surfaces became more regularly curved in the successive layers forming the dome, which can be taken apart and examined separately.

The circumferential reduction has been met, firstly, by the domical uprise, and, secondly, by the wavy peripheral undulations and the longer peripheral folds seen to the right hand in fig. 1, Plate XVIII.

The process of plagioclinal mountain-building so interestingly worked out by Dr. Callaway in the Wrekin[1] seems to have occurred somewhat in the foregoing manner.

[1] 'On Plagioclinal Mountains' (*Geo. Mag.* 1879, pp. 216–21), and other papers in the *Q. J. G. S.* Dr. Callaway considers that the core of the Wrekin and of similar anticlinals is a wedge of rocks driven up from below along lines of fault which have a direction oblique to the strike of the complex. According to my views, this wedge or tongue may be forced up by lateral compression, and the faulting may result from the same cause. It is really a question of dominance in the direction of the lateral pressure. If the dominant pressure is at an angle to, but in the direction of, the strike of the basal rocks, the core out of which they are formed will have its beds striking across the resultant

EXPLANATION OF PLATE XVII.

Fig. 1. Diagram explanatory of Experiment No. 13. The dotted circle represents the original size of the disc of clay; the thin line circle, the disc after compression. The smaller circles are the domical folds induced by circumferential compression, truncated against the metal band. See photographs on Plate XVI and explanation of that Plate.

Fig. 2. Diagram explanatory of Experiment No. 14. Outside dotted circle shows original [illegible] of bottom bed of clay upon which was moulded a slight elliptical [illegible] No. [illegible]. The [illegible] bed was divided with a knife into beds [illegible]. The inner lines represent the plan of upper surface of [illegible] after removal of the domed upper layer. [illegible] the [illegible] the bed [illegible] altered [illegible].

Fig. 3. [illegible] the model, showing the relation of the beds and domes when compacted [illegible].

This experiment proved that [illegible] plasticity, a core of rocks having a strike diverging from the axis of the resultant anticlinal might be forced up by lateral pressure acting [illegible] direction of the strike of the rocks forming the core. See Plate XVIII and accompanying explanation.

Figs. 4, 5 and 6 [illegible] showing how the [illegible] was obtained in Experiments Nos. 15, 16 and 17 [illegible] the several [illegible] forms [illegible] upon [illegible] the compression.

Figs. 7, 8 and 9. Diagrams [illegible] folding [illegible] Experiment No. 18.

See [illegible] photographic plates referred to [illegible] Experiments Nos. 15, 16 and 17.

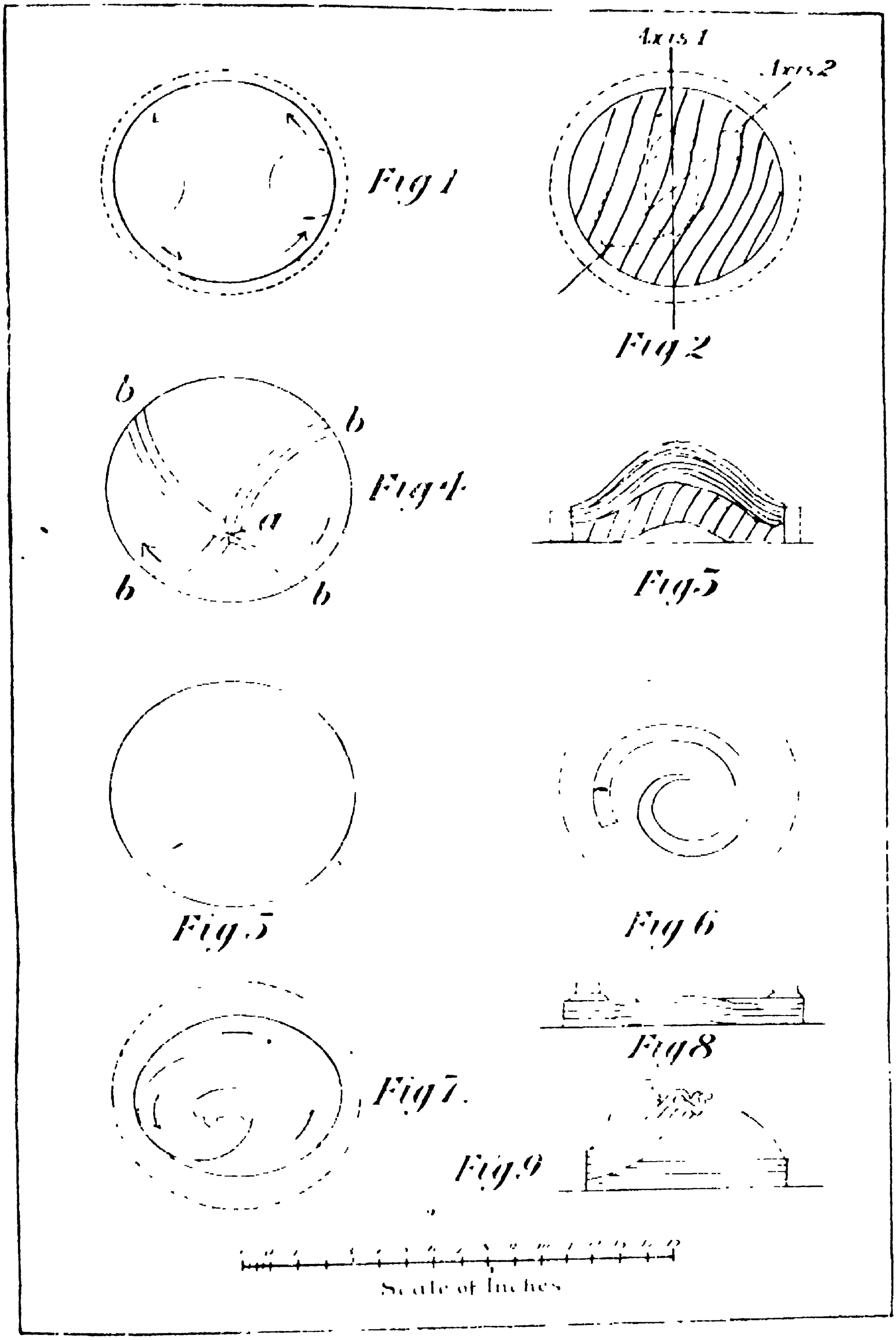


Fig 1
Axis 1
Axis 2
Fig 2
b
b
Fig 4
a
b
b
Fig 3
Fig 5
Fig 6
Fig 8
Fig 7.
Fig 9
Scale of Inches

The main difference is, that instead of being acted upon by equal multilateral or circular compression, the forces that upheaved the Wrekin were dominant along the shorter axis.

Explanation of Plate XVIII.—Fig. 1 shows the whole compound structure complete, as taken out of the compressor.

Fig. 2 shows the three top layers removed, the lower layer remaining on the base bed.

Fig. 3 shows the whole of the layers or concentric domes removed, and exposes the irregular surface of the base bed, with its oblique bedding having a high dip.

The dark surfaces are due to the black sand, as already referred to.

Experiment No. 15 (Plate XIX.): *Torsion Structure.*—This experiment was conducted with the object of testing the possibility of torsion structure arising from the compression of a combination of solid plates.

It was discovered by the experiment detailed in p. 138 that a flexible developable surface of paper of octagon form, as illustrated in the model Plate X., on being compressed at the edges, developed a spiral movement in the centre, made manifest by the movement of the attached index finger.

In the circumferential compression of clay discs about to be described it was found in some cases advisable to initiate a desired movement by giving

anticlinal. As to whether the core is formed by faulting or 'moulding' will depend largely upon its rigidity.

the structure what I term, for want of a better name, an 'initial bias.'

In this case (Experiment No. 15) the initial bias was obtained by placing strips of clay of a triangular section upon the oiled metal bed-plate. These radiated in curved lines from a fixed centre, '*a*,' which was eccentric to the true centre of the circle (see Plate XVII., fig. 4).

Upon this bed-plate with curved radial ridges, *b*, *b*, oiled on upper surface, was placed a sheet of clay, $\frac{1}{8}$ of an inch thick, carefully moulded to the underlying ridges. This sheet was dusted over with very fine sand[1] and two other sheets superposed.

The assemblage was placed in the compressor, and was gradually screwed up until the base became $7\frac{3}{8}$ inches on the longer and $7\frac{1}{8}$ inches on the shorter diameter.

The upper part of the structure, not being so much compressed, was somewhat larger than the base.

To determine the torsional movement (if any) that might take place, straight lines were pricked across the surface of the model.

Folds arose on the periphery at four points, the terminations of the radial ridges, *b*, *b*, *b*, *b*. These folds had overthrows to the left, and the finished model (Plate XIX., fig. 1) indicates a general spiral

[1] The sand used throughout was obtained from the washings and siftings of glacial clays of various degrees of fineness. The surfaces to be photographed were also dusted over, as it was found the pictures came out better when so taken.

PLATE XVIII.

FIG. 3.

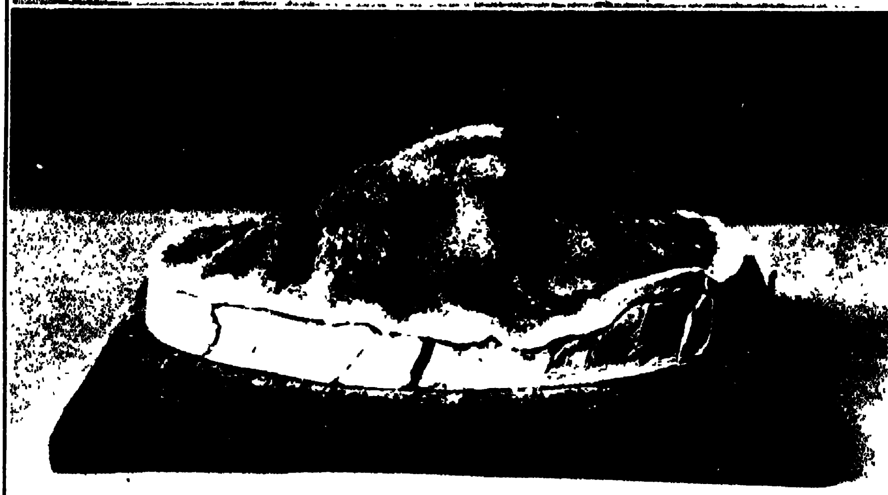


FIG. 2.

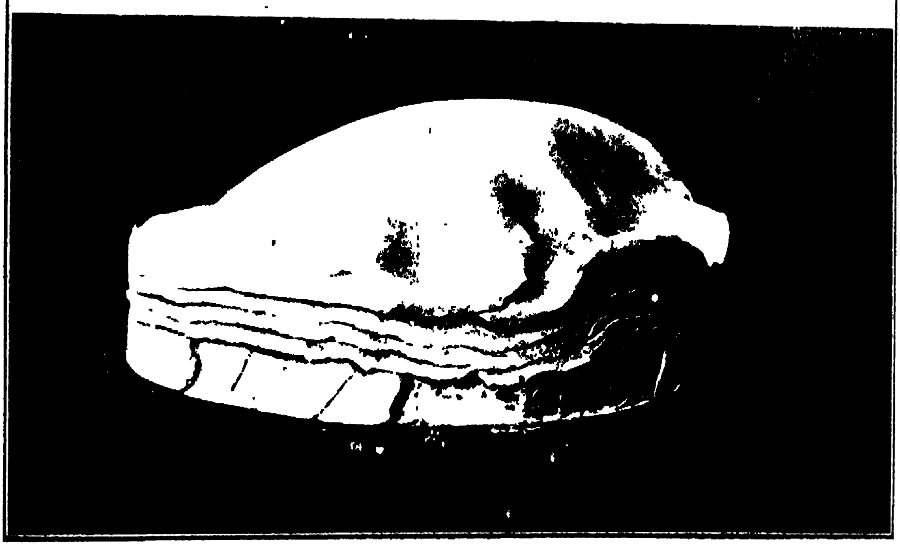


FIG. 1.

movement to the left, as do also the curvatures of the pricked lines.

The model is not easy to describe, but it consists of four intersecting overfold-anticlinals, or ridges which rise to their highest point at the *locus* of intersection. It may be called a ridged-dome uplift. The finished height is $3\frac{3}{4}$ inches. The centre and the folds are hollow. The domical lift left the initial ridges on the zinc bed-plate, their function being simply to give direction to the movement. The sharp bending of the folds has fissured some of them at the ridges.

Explanation of Plate XIX.—Fig. 1. Model as taken out of the compressor, with the assemblage complete. Viewed from the side nearest the excentric centre.

The fold on the opposite side (unseen) is more perfect in form, and is larger than that seen.

Fig. 2. View with top plate removed.

Fig. 3. View of the bottom plate or stratum.

It will be noticed that the folds of the bottom plate are in the instances given sharper and more abrupt than the overlying folds, the upper-layer curvatures being the easiest. This seems to be a general rule.

Experiment No. 16 (Plate XX.): *Longitudinal or Linear Folding on a Circular Plate.*—In this experiment the aim was to discover whether circumferential pressure was competent to produce a longitudinal or linear fold.

With this object the 'initial bias' was provided

by a slight core of the shape represented in fig. 5, Plate XVII. Upon this were moulded, as in preceding experiment, three layers of clay measuring together ½ inch thick.

On compression this disc became slightly oval, the diameter on the line of the ridge measuring 9 in., and that at right angles thereto 8¾ in.

The apparatus was, of course, not screwed up to its full extent.

A longitudinal fold was developed, and extended right across the circular plates. One end of this terminated domically, like the ridge on lead sink (Plate V., fig. 2); the other end, the nearest to observer (fig. 1, Plate XX.), terminated at the zinc belt against which it was truncated.

This development was due to peripheral folding, which generally occurred somewhere between the handles of the compressor.

The drawing in of the belt seems to cause an unequal pressure, and consequent squeezing or flow of the clay to this point of the circumference.

The layers of clay as they rose in anticlinal form left the core on the zinc bed-plate.[1]

A spiral movement to the left accompanied the development of the fold.

This experiment clearly proves that converging pressure, such as is set up by expansion on a rise of temperature, is competent to produce a longitudinal or linear fold. The conditions precedent seem to

[1] In some cases portions of the core became nipped in and carried up with the fold.

PLATE XIX.

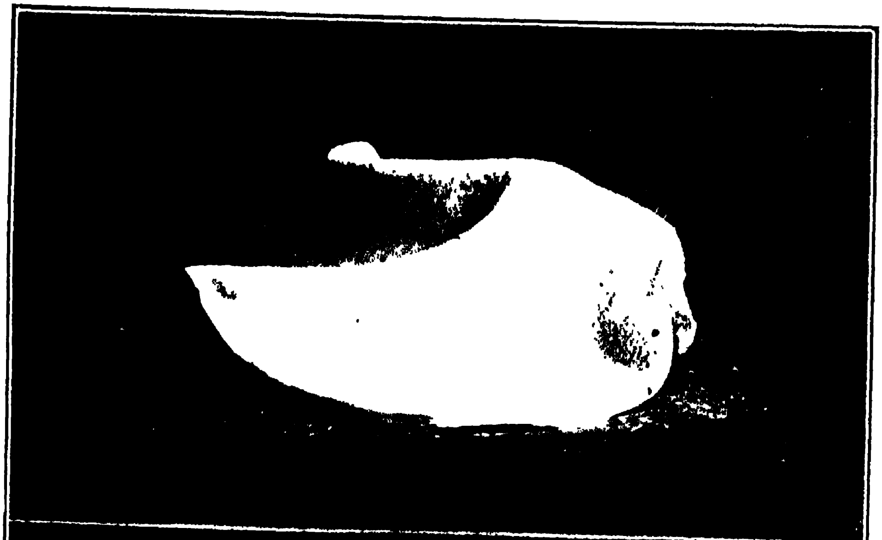


FIG. 3

FIG. 2

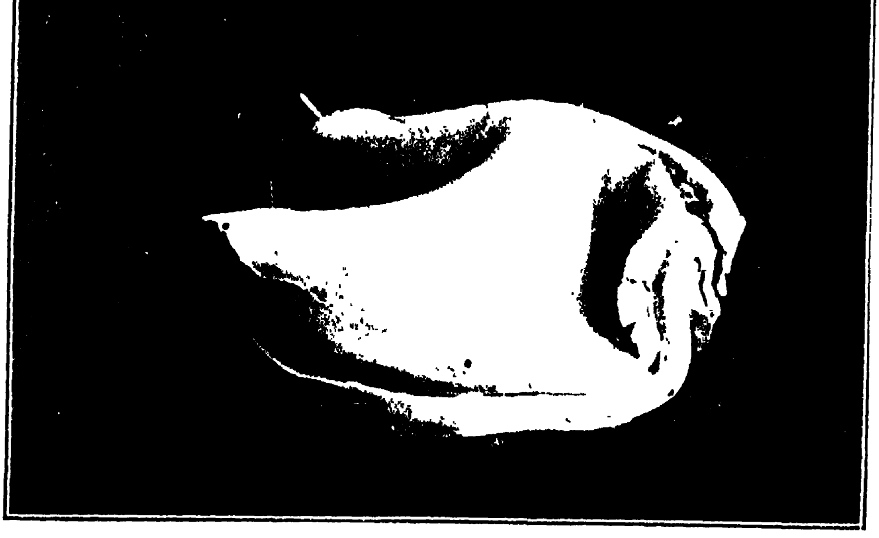


FIG. 1

PLATE XX

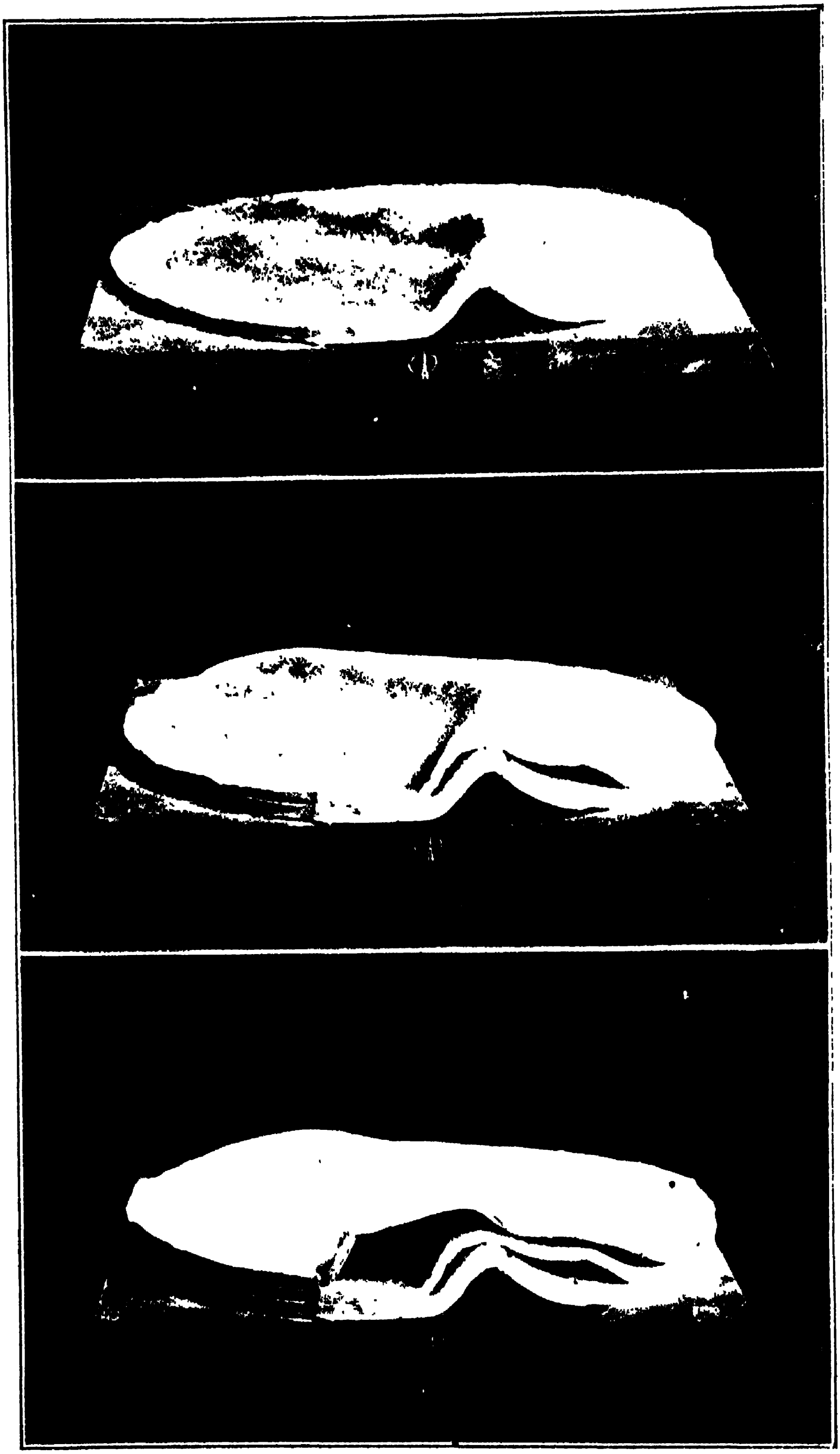


Fig. 3

Fig. 2

Fig. 1

be a weak line in the strata and a large area of expansion compared with the thickness of the strata.

Explanation of Plate XX.—Fig. 1. Finished model as taken out of the compressor, showing truncation of linear fold against the zinc band.

Fig. 2. Model with top stratum removed.

Fig. 3. Base stratum.

Experiment No. 17 (Plate XXI.): *Compression of Clay Plate moulded over a Helical Core.*—This may be designated a 'sporting' experiment.

A helical core was laid down on the metal bed-plate, and a disc of very plastic clay, ⅝ inch thick, laid upon and moulded to it (fig. 6, Plate XVII.).

This homogeneous plate behaved in a singular and unexpected manner. On being compressed to the fullest extent the machine was capable of, it became in general form like a ram's horn, measuring 7½ inches on the longer diameter and 6⅝ inches on the shorter (see fig. 1, Plate XXI.). The periphery thickened from 1½ to 2 inches, making the under-side of the model cup-shaped, averaging about 1¼ inch deep. The coil or helix became rounded in section, and a similar coil was developed on the underside, apparently independently of the top one. In cross-section the two helices are anticlinals, and the hollow between a 'synclinal'[1] (see fig. 2, Plate XXI.).

Lines were drawn at right angles across the

[1] When the model is reversed the two anticlinals become synclinals and the synclinal an anticlinal.

circle before compression, which after compression showed a spiral movement from left to right, or in the opposite direction to that of preceding experiment.

This model was the result of a series of complex movements very difficult to analyse. It appeared that the helical form gave it great strength to resist the circumferential pressure, which turned down and thickened the periphery of the plate, at the same time developing the helical folds and producing the cup form of the underside.

Although not simulating any known earth structure, it shows what infinite possibilities of change of form by compression may exist in the earth's strata.

Explanation of Plate XXI.—Fig. 1 is a view of the model as it came from the compressor, with the ammonite-looking coil developed on top surface.

It will be observed that the bottom rim is irregular on the edge, which is pressed up in places in a ragged fringe.

Fig. 2. View of the *underside.* The internal fold is part of the helix which was developed on the underside of the model. It is the convex side of the synclinal. This view shows well the turning down and thickening of the peripheral rim. The rounded form of the cross-section of the helix is evidently produced by folding.

To describe the model in a phrase, it may be designated 'a helical folding.'

Experiment No. 18 (Plate XXII.): *Spiral Fold-*

PLATE XXI.

FIG. 2.

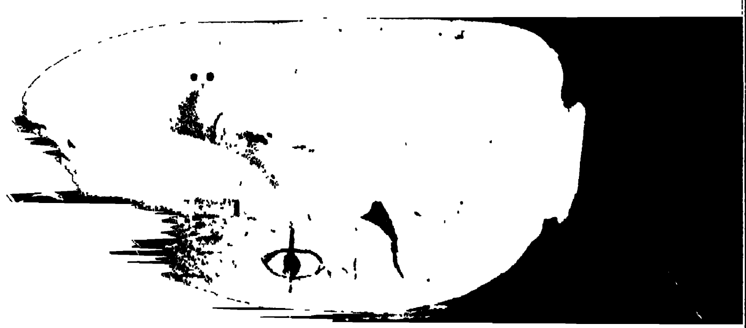


FIG. 1.

ing and Shearing.—The object aimed at in this experiment was to produce a domical uplift by thickening in the centre, which it utterly failed to do; but the result was most interesting. A circular lenticular core, $3\frac{1}{4}$ inches in diameter and $\frac{1}{4}$ inch thick in the middle, was placed on the oiled base-plate, and three layers of clay, $\frac{1}{4}$ inch thick, separated by sand dustings as before, were successively moulded over it. A fourth was then put on and shaved to a level surface with a knife, as in section, fig. 8, Plate XVII.

Lead weights embedded in clay at the base were then placed round the periphery, with the object of keeping down the peripheral folding and forcing the clay to thicken in the centre.

On screwing up the assembled strata in the compressor it was found impossible to prevent peripheral folding, and after several vain endeavours to do so the weights were removed and a compound fold was allowed to develop.

After a certain amount of tightening of the band the whole series of four strata-plates sheared, and the portion of the series on the left hand of the shear overrode that on the right, moving spirally upwards and producing a clean-cut shear-plane. All this is well shown in the photograph, Plate XXII. The peripheral fold was carried up by the spiral movement.

The length of the peripheral shear, measured when dry, was $7\frac{3}{8}$ inches.

The machine was screwed up to its limits,

resulting in a reduction of the base stratum to $7\frac{1}{2}$ inches on the longer and $6\frac{3}{4}$ inches on the shorter diameter (fig. 7, Plate XVII.).

The underside of the overriding series was slickensided concentrically with the periphery.

The central part, instead of keeping down and thickening, rose very sharply on a definite axis in an oval dome; and this again partook of the spiral movement, becoming at one end, as it approached the tighteners, what may be termed a spirally folded dome with an almost vertical side.

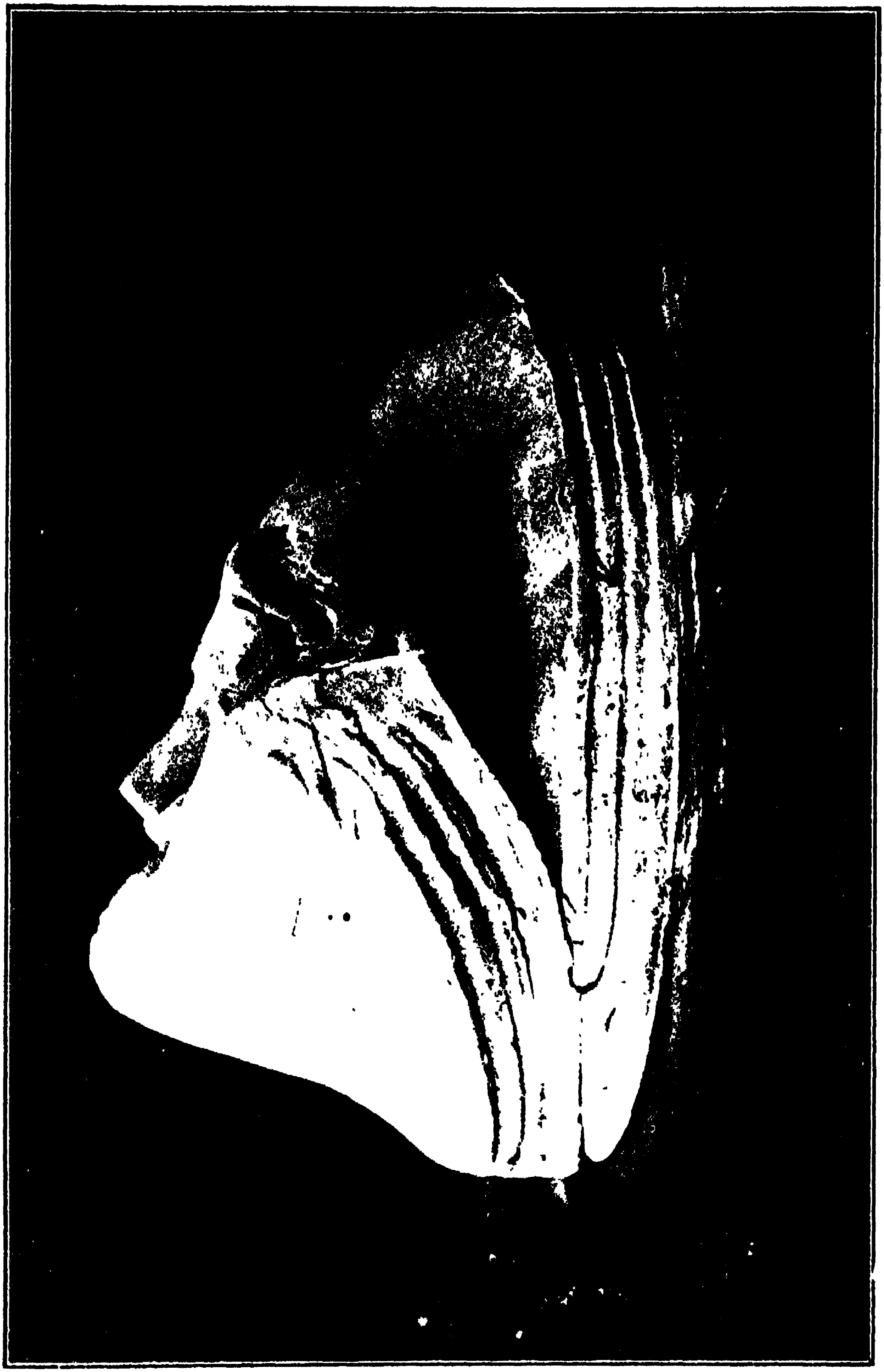
In this experiment the clay was as stiff as could be conveniently worked.

This proved one of the most instructive experiments of the series, and shows the vast possibilities of earth structure under centripetal compression-stresses varying in strength and direction.

Explanation of Plate XXII.—Completed model as taken out of the compressor. It shows the shear-plane is of a screw or spiral form. The strata to the left override those to the right, the peripheral folds being carried up on the end of the strata (shown also in fig. 9, Plate XVII.). The dome is folded on itself round the apex, the surface of the dome to the left hand being much steeper than that to the right. The height of the apex is $5\frac{1}{2}$ inches. Diameter of base, $7\frac{1}{2}$ and $6\frac{3}{4}$ inches respectively.

This is technically a right-hand screw.

Experiment No. 19.—It is unnecessary to describe this experiment, as the model was broken



up as soon as made, not being thought worth preservation.

Experiment No. 20 (Plate XXIII.).—A small lenticular core with eight radial arms, as shown in fig. 1, Plate XXIV., was placed upon the oiled bed-plate. These radii were thicker at the periphery than at the centre, thus sloping inwards towards the centre. Upon this arrangement four layers of clay, averaging from $\frac{7}{8}$ to 1 inch thick when combined, were laid and moulded, the layers being separated by sand dustings as before (figs. 2 and 3, Plate XXIV.).

On screwing up this assemblage in the compressor, folds began to develop over the radial arms, increasing in amplitude towards the periphery. The centre began to rise and the radial folds were carried up with the dome. A line of weaknesses developed itself along the axis *a b*, fig. 1, Plate XXIV., which rose in a sharp ridge. At each end of this axis the folds became complex overfolds, and the ridge, subject to end pressure as the band tightened, became curved in plan.

With the object of ascertaining the structure of the interior of the dome, it was when dry sawn across near the centre at a slight angle. The complexity of the overfold at one of the peripheral ends of the ridge is well shown in fig. 1, Plate XXIII.

The central folding of the four layers as disclosed by the saw-cut is shown in fig. 2, Plate XXIII., and it is remarkable how perfectly they

are bent, retaining an even thickness without fracture. This is an example of the remarkable strength possessed by a dome structure, as each layer was not more than $\frac{1}{4}$ inch thick. Although the clay was quite soft, each layer acted independently of the other, the outer layer rising the highest and a cavity occurring between them. These points are well shown in fig. 2, Plate XXIII. The section was sawn with a small handsaw when the model was dry.

Explanation of Plate XXIII.—Fig. 1. View of the completed model as taken out of the compressor. The complex overfold, the curved ridge or axis, and the final form taken by the radial flanking folds explain themselves. The base plan was $7\frac{7}{8} \times 6\frac{1}{2}$ in.; the ridge, $8\frac{7}{8}$ in. long, overhanging the compressor; apex of ridge, 4 in. high.

Fig. 2. View of section sawn across the ridge of dome, looking at it from the same direction as Fig. 1. The sharp curvature of the base stratum is here well shown.

EXPLANATION OF PLATE XXIV.

FIGS. 1, 2, 3.—Diagrams explanatory of Experiment No. 20. No. 1 is a plan showing radial arms and central boss to give 'initial bias.' Fig. 2 is a section on line *a b*. Fig. 3, elevation of the periphery. See Plate XXIII.

FIGS. 4, 5, and 6.—Diagrams explanatory of Experiment No. 22 to illustrate spiral shearing. Fig. 4 shows in dotted lines the original size of the disc of clay; the firm lines show plan of same after compression. Figs. 5 and 6 are side-elevations, showing the screwing round and spiral shearing of the plate by circumferential compression. See Plates XXVI. and XXVII.

FIGS. 7, 8, 9, and 10. Diagrams explanatory of Experiment No. 23, showing the several phases of spiral shearing of a homogeneous clay disc acted upon by the circumferential compressor. For details see description of the Experiment No. 23 and photographs, Plates XXVIII. and XXIX.

PLATE XXIII.

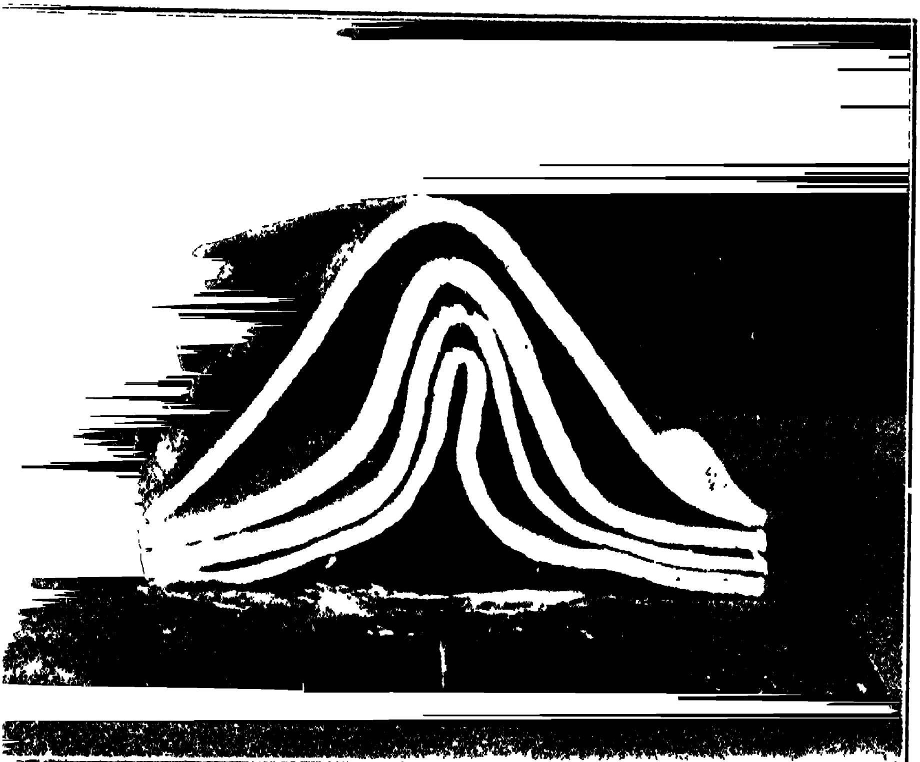


FIG. 2.

FIG. 1

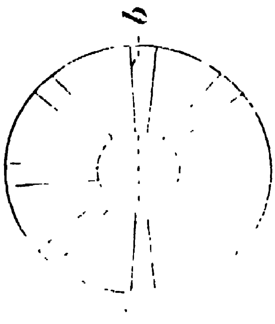


Fig 1

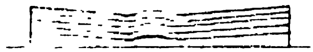


Fig 2

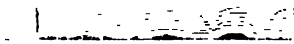


Fig 3

Fig 4

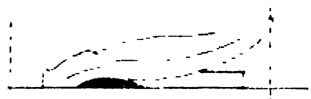


Fig 5

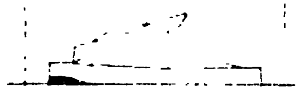


Fig 6

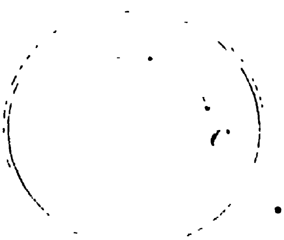


Fig 7

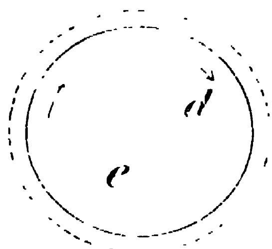


Fig 8

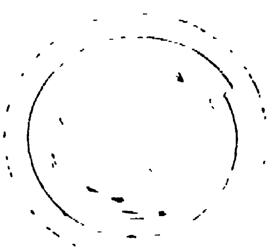


Fig 9

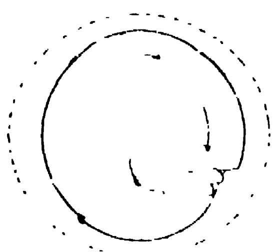
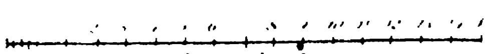


Fig 10



Scale of Inches

Experiment No. 21 (Plate XXV.).—This was practically No. 20 repeated, but with more pronounced radial arms, seven in number, having a greater initial slope towards the centre. Two layers of clay, $\frac{5}{8}$ in. thick each, were moulded over the radii as in No. 20, the surface being sand-dusted.

The result was a very decided peripheral folding over the radial arms. Each fold was a more or less perfect ellipsoidal truncated dome, the longer axes being radial. The centre over the lenticular core rose in a small axial dome or boss, from which the radial folds were separated by synclinal hollows, excepting in one instance, where the central uprise became an irregular continuation of a radial fold.

There was a tendency to lift at the periphery on screwing up the apparatus, instead of a general doming up of the whole assemblage as in previous cases.

This experiment is a purely artificial one, made with the object of analysing the movements of clay plates under certain exceptional conditions. They may never occur in nature.

Explanation of Plate XXV. View showing peripheral folding in the form of truncated ellipsoidal domes radially arranged.

The axis of each fold, of which there are seven, is irregularly radial. The small domical uprise in the centre is seen.

Experiment No. 22 (Plates XXVI. and XXVII.): *Spiral Shearing.*—A single plate of clay, $\frac{5}{8}$ in. thick,

was placed in the compressor. No bias was introduced in any form, excepting what might naturally have come about in rolling out the clay.

The result was most astonishing. On drawing in the band the clay began to shear and screw round on a spiral shear-plane of low inclination. As the screwing up proceeded the upper portion rotated rapidly to the right over the lower portion until the shear-plane measured 8½ inches on the periphery. The upper surface assumed a truncated domical form, the pointed end of the screw being 3 inches high above the metal base-plate (see figs. 4, 5, and 6, Plate XXIV.).

The underside of the overriding plate is beautifully slickensided.

The circumference of the plate before compression measured 2 ft. 6½ in. After compression, 1 ft. 10½ in. = difference 8 in.

The surface of the top was in many places sheared into thin layers in spiral directions following the main shear.

The clay in the last three experiments was rather soft.

This was a most satisfactory and instructive experiment, demonstrating that massive beds responding to centripetal pressure under certain conditions shear in spirals. A combination of thin beds subjected to similar pressure would, as we have seen, and will see again in Experiment No. 23, develop peripheral folds before shearing.

This was technically a right-hand screw.

Explanation of Plates XXVI. and XXVII.—Plate XXVI. View showing the moving thin edge of the overshear, which is beautifully slickensided on the underside. The clay plate where not sheared now measures $\frac{3}{4}$ inch thick—that is, it has undergone a thickening of $\frac{1}{8}$ inch.

Plate XXVII. View looking towards the point of the overshear, showing the unsheared clay plate forming the base and the portion of the overriding clay, or '*overshear*,' where it has left the *undershear* and been carried forward to the right by the peripheral movement.

Experiment No. 23 (Plate XXVII.): *Spiral Shearing* (*continued*).—A homogeneous plate of clay, somewhat stiffer than in Experiment No. 22 and $1\frac{1}{4}$ inch thick, was prepared and put into the compressing band.

On screwing up the clay began to thicken and shear from the periphery to the centre, opposite the tightening-screw, as shown at *c* in diagram, fig. 7, Plate XXIV. This developed into a convex rise as shown at *d*, fig. 8. At the same time a lunette anticlinal began to show itself at *e*, fig. 8, following the periphery, and the overshear began to lift. The spiral movement was in this case to the left, or in the opposite direction to that of the preceding experiment.

The third phase, as shown in diagram, fig. 9, was an intensified form of that exhibited in diagram, fig. 8.

Finally the shear-plane became well developed,

as shown in Plates XXVIII. and XXIX., the point of the spiral rising 5¼ inches above the base. The overshear and the undershear were well slickensided. The shear was more abrupt and not so regular as Experiment No. 22.

The underside of the compressed disc of clay was found to be a volute-formed hollow, the result of combined compression and torsional movement, the complement of the convexly screwed upper surface.

The finished base (fig. 10, Plate XXIV.) measured when dry 7 by 6¾ inches.

Explanation of Plates XXVIII. and XXIX.—Plate XXVIII. A photographic view of the finished model as taken from the compressor, showing the surface form assumed by the disc and the upper surface form and rise of the overshear, together with the numerous minute screw-shears which developed on the surface of the clay.

Plate XXIX. View of the same model taken from a point opposite the shear-plane.

The original plate of clay thickened about ¼ in., one of the results of compression.

Experiment No. 24 (Plate XXX.): *Spiral Domical Overfold.*—Three plates of clay, averaging together ¾ inch thick, or ¼ inch each, were laid upon three small lenticular cores arranged in the form of a triangle on the metal bed-plate; but these cores did not seem to influence the movements.

On screwing up a fold began to develop near

PLATE XXX.

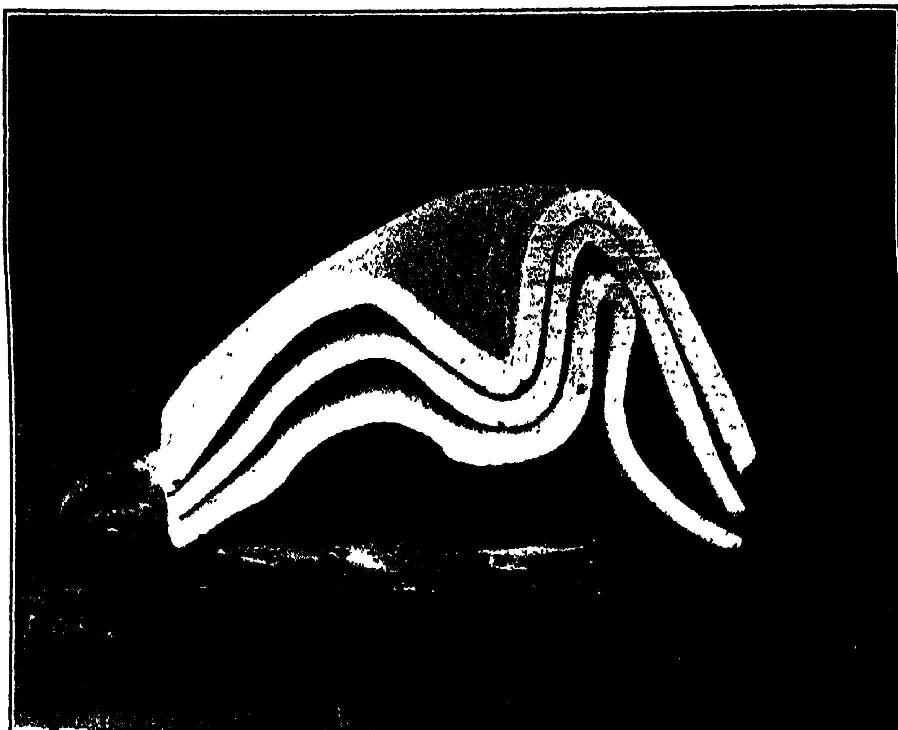


FIG. 2.

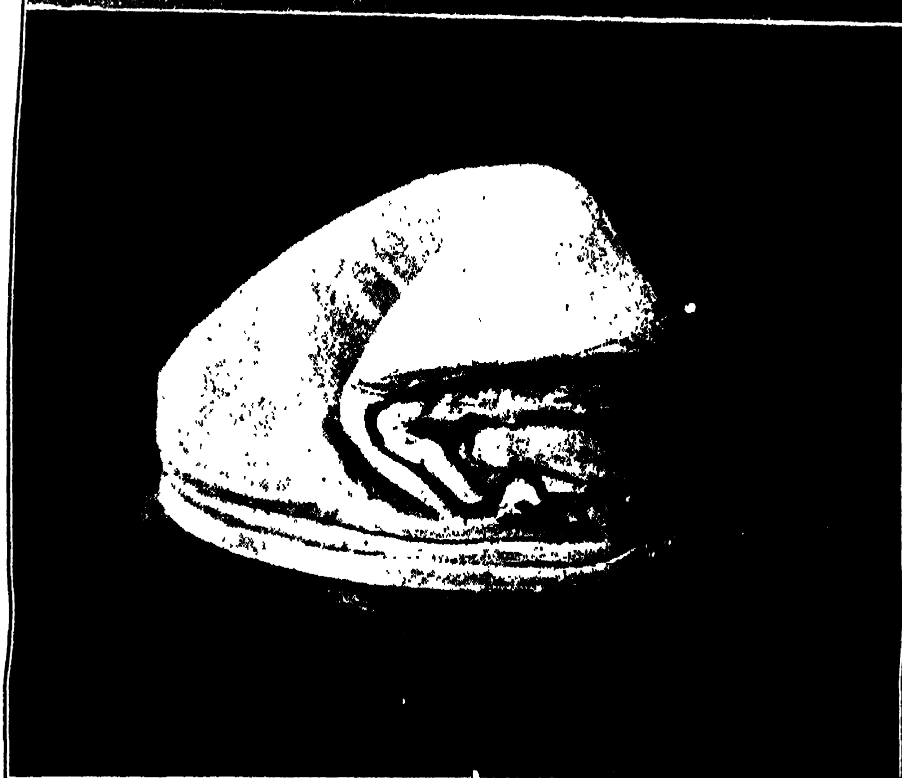


FIG. 1.

the tightening-screw, the centre and plate generally rising as a dome.

The result may be described as a peripheral overfold-dome, the domical fold rolling over, with beautiful regularity, during the screwing up of the band.

The overfold-dome is well exhibited in the view (Plate XXX., fig. 1).

The perfection and harmony of the movement of the clay plates in adapting themselves to the new conditions and complicated bendings and bucklings are shown in fig. 2, which is a saw-cut section along a line a little oblique to the shorter axis of the oval.

This experiment proves very clearly that the centripetal pressure, and consequent reduction of the radius and circumference of the original discs, are met in two ways: the radii, being shortened, arch up in dome form, and the reduction of the circumference brings in peripheral folding. The result is a spirally folded dome.

Explanation of Plate XXX.—Fig. 1. View of model as taken out of the compressor, showing the peripheral overfolding of the dome.

Fig. 2. Saw-cut section across the dome looking from the same direction, or roughly parallel to the overfolding. Final size of base, $7\frac{5}{8}$ in. × $6\frac{1}{2}$ in.; height, $3\frac{7}{8}$ in.

CHAPTER XVI

WHAT THE EXPERIMENTS IN CIRCUMFERENTIAL COMPRESSION TELL US

THE experiments described in the previous chapter enable us to understand the effect of multilateral pressure upon an assemblage of diverse and bedded strata.

It has been shown clearly by more than one geologist that such multilateral or, as I term it, circumferential compression has taken place in nature. From these diverse lateral pressures some extraordinary effects have resulted, such as the spiral twisting or screw movements of bedded rocks, similar to those recorded by Mrs. Gordon, D.Sc., in her very clever paper on 'The Torsion Structure of the Dolomites.' [1]

I have, I think, shown both in 'The Origin of Mountain Ranges' and in this work that all the tangential movements of compression in the earth's crust are, in a greater or less degree, multilateral.

From whatever cause this multilateral compression arises, it is guided in its effects by the existence of lines of resistance and of weakness. We have seen that the provision of some simple contrivance to

[1] *Q. J. G. S.* 1899, pp. 560 633.

give what I have called an 'initial bias' to the movements often makes a wonderful change in the form of the model that comes from the compressor.

In the case of the Dolomites and similar mountain structures the multilateral compression is, as we may say, focussed or concentrated in a comparatively small area, though it may originate in a large one.

The experiments have satisfied me that pressure in three directions at right angles to each other—for this is what the compression resolves itself into—is competent, under conditions that may easily happen in nature, to produce a vast variety of unexpected forms. These forces may be concentrated in a small area, producing a rocky whirlpool, or may be spread out and taken up by long folds, which may seem to be the result of simple unilateral or bilateral pressure.

When we investigate the individual folds of strata which have been subjected to lateral pressure, we find them in their undisturbed forms to be, almost without exception, boat- or canoe-shaped, proving, as I have frequently pointed out, that the pressure producing them has acted centripetally—otherwise multilaterally.

The axis of a mountain range is seldom or never in a straight line. It curves and sweeps round to right or left, as the case may be, the effect of forces acting in the direction of the major axis simultaneously with those acting transversely; or it may be, and often is, crescentic in plan. This is

the general rule, but exceptions can be quoted where the lateral pressure has acted circumferentially more equally from the surrounding area. Stefani, in his monograph on the Apuan Alps, says that the most simple folds in their more regular courses answer to ellipsoids, and the horizontal sections of their strata to ellipses, but that the folds often bifurcate and ramify with the utmost irregularity. The horizontal course of a fold may deviate in all possible directions up to right angles. It may turn to the same part of the horizon from which it starts, and describe a semicircular, or horseshoe, or any other curve, even having a very short radius. In a vertical direction a fold may divide in every possible way—that is, be inverted on one side of a region, or in an opposite direction in another, or elsewhere become vertical.

The predominating direction of the folds of the Apuan Alps is, however, in the direction of the meridian, where the prevailing compressions show themselves; therefore the compressing forces had the principal, but not the exclusive, direction of the parallels between east and west.[1]

If the lateral forces have a dominant parallel direction, the axis of the range will be approximately at right angles to them. Once a linear folding or corrugation is established, it strengthens the strata enormously as against an end-compressing force; hence the distortion of the individual folds in the efforts of end compression. A careful

[1] *Le Pieghe degli Alpi Apuane*. Firenze.

study of the models from the compressing-band and the accompanying descriptions will enable us to understand more clearly the effect of multilateral compression on cubical masses, as differentiated from the unilateral folding of sheets or bars.

It is a mode of investigation calculated to teach us much of movements of a complexity otherwise difficult to follow.[1]

[1] Being invited by the Cotteswold Field Club at one of their excursions in 1902 to give my views of the origin of the Woolhope Dome, I reproduce here what I then said, as it bears directly on the subject of this chapter:—

'Imagine a great thickness and extent of horizontal strata, subject to compression either by internal expansion or outside pressure. Mechanical reasoning as well as experiment shows that when the pressure reaches a critical point the strata will give way by folding in the weakest places. These weakest places I have satisfied myself by experiment are determined mainly by the form and inequalities of the floors upon which the strata have been deposited; hence the folding may take a circular, an elliptical, or a linear form. But whatever form it takes, sections in any direction will show the bedding more or less curved.

'If the plan of the uplift be a circle or an ellipse, two sets of stresses predominate—the radial and the peripheral. The radial pressures are relieved by the uprise of the strata in domical form; the peripheral, by folding or shearing. This is the inevitable result of the reduction of circumferential girth, unless, indeed, the peripheral portions could give way by simple thickening, which is unlikely. Let us apply these principles to the Woolhope Dome. There are positive evidences of the uplift being due to converging pressure. The axis of the dome is north-west and south-east. It is pear-shaped, the convex head of the pear being to the north-west. In accordance with the principle enunciated, a reduction of the circumference by folding has taken place. The one inch to the mile geological map shows this clearly by the horizontal folds therein depicted. This horizontal movement is further emphasised by the fault cutting the head of the pear, which strikes in a north-easterly direction, shifting the rocks by a horizontal throw.

'The Malvern folding, lying to the east of the Woolhope Dome, is on a north and south alignment, no doubt influenced by an earlier

Not only may folds be screwed round by differentiated multilateral pressure, but the torsional movement may expend itself in spiral shearing. The movement may commence in folds, that gradually work round the axis until fracture takes place and the screw shear-surface is established. Nor must it be lost sight of that the folding and faulting met with in mountain ranges are not all of an age. If in one stage of the building the strata are tightly compressed by folding and lateral pressure, the next set of pressures can only find relief in overthrust faulting. This is well shown by Stevenson in a paper on 'The Faults of Southwest Virginia,'[1] where two faults occur having a maximum length of 130 miles, their vertical throws varying along their respective lengths from 10,000 feet to 500 feet. Fifteen faults are enumerated, and between them are often found sections displaying the well-known canoe-shaped overlapped anticlinals and synclinals which distinguish the Appalachian folding.

Moreover, the faults in their courses do not follow straight lines, and many of them, notably the Saltville fault, exhibit a total indifference to

displacement and the form of the rock-floor upon which the Silurians were laid down. Here again horizontal folds, due to pressure acting at different angles, are to be traced. Thus it is seen that uplifts having different axial directions may take place contemporaneously and at no great distance from each other.

'The questions involved are very intricate and most interesting, but as a generalisation I may say that, taken on a more extended scale the great formations tend less to assume domical forms.'

[1] *American Journal of Science*, vol. xxxiii. 1887, pp. 262–70.

that of the anticlinals and the strike of the rocks.

Stevenson from these very practical observations infers 'that the faults are of later date than the system of folds, and that they may have been produced at a time, possibly, as late as the era of Mesozoic disturbance marked by dykes throughout the Triassic area of the Atlantic borders.'

Regular symmetrical ellipsoidal folding is, I fully believe, the result of converging pressures acting simultaneously. It is quite impracticable to imagine that these curved forms can be the product of two sets of forces acting at different times. The first parallel folding would give such a strength to the strata that any pressure energetic enough to move the rocks in a direction normal to the first folding would inevitably end only in fracture and dislocation.[1]

In 'A Report on the Geological Structure of a Portion of the Rocky Mountains' Mr. R. C. McConnell gives a very interesting section exhibiting several types of mountain structure.[2]

[1] Lord Avebury, in his charming book on *The Scenery of England, and the Causes to which it is due*, refers to the cross-folding of the Alps, and explains it as the result of two forces acting simultaneously at right angles to each other. Two forces acting thus would practically resolve themselves into multilateral compression. This, I venture to think, points to expansion as the cause of the pressure. The illustrative diagram does not accurately represent what would take place in a sheet compressed by two forces at right angles to each other, as is pretty well proved by the compressive experiments already recorded in these pages, but it illustrates Lord Avebury's meaning.

[2] *Geological and Natural History Survey of Canada.* Pt. D: Annual Report, 1886.

This section is from west to east, in the vicinity of the Canadian Pacific Railway, and near the fifty-first parallel. Its length is about sixty-three miles. The Rocky Mountains in this latitude 'are divided by radical differences in structure into two distinct geological provinces, the line of division being nearly coincident with the western base of the Sawback Range.' The region east of this line has been broken by a number of nearly parallel fractures into a series of oblong orographic blocks, which have been tilted and pushed one over the other in the form of a westerly dipping compound monocline. There are seven principal faults in the district examined, and six well-defined blocks, resting upon one another, in regular succession from west to east.

It is inferred that the thrust producing these movements and dislocations came from the west, and must have been highly energetic, as some of the breaks are of huge proportions, accompanied by displacements of many thousand feet.

The tilted blocks form a series of nearly parallel ridges, somewhat resembling the structure of the Great Basin, though arising from compression and one block overriding another, and not, as in the Great Basin, from successive downthrows.

The section shows Cambrian and Devonian rocks superimposed upon the Cretaceous of the foothills, with, in one case, an estimated vertical displacement of 15,000 feet and a horizontal move-

ment of the Cambrian beds of about seven miles. The angle of inclination of the thrust plane to the horizon is very low, and in consequence its outcrop follows a very sinuous line along the base of the mountains.

In the western part of the chain, between the Sawback Range and the Columbia, the structure is entirely changed: no reversed faults have yet been recognised there, and ordinary and overturned folds play the most important *rôle*.

'The constituent formations of the two regions, as well as their structure, are very dissimilar, and some of the formations when traced westwards become greatly changed.'

Here I must be permitted to digress, and point out that this section of the Rocky Mountains only emphasises the connection of great dynamic movements with sedimentation. The section of the Western Province shows a thickness of 23,000 feet, the greater part of which is Cambrian. The Eastern Province has, in addition, a series of rocks, including the Cretaceous, resting upon the Cambrian and combinedly estimated at 18,000 feet. These great lateral movements were, of course, post-Cretaceous so far as the Eastern Province is concerned. Probably the Cretaceous rocks originally covered the Western Province, and have been since swept away by denudation.

It is also probable that the movement was multilateral, the dominant force acting from the west. Mr. McConnell concludes thus: 'The portion

of the Rocky Mountains examined in the construction of the accompanying section is thus characterised in its eastern part by a series of great fractures and thrust-faults, in the centre by broad sweeping folds, and in the west by folding and crumpling, accompanied by the development of cleavage planes and a limited amount of metamorphism.

'Among its other more important features may also be noted the absence of recognisable unconformities, the absence of any of the older crystalline schists, the relatively smaller amount of disturbance in the central parts of the range than towards the edges, the want of similarity in the sequence of the formations east and west of the axis, and the marked preponderance of calcareous beds between the Middle Cambrian and the Cretaceous.'[1]

Plate No. XVIII., representing the effect of simultaneous circumferential compression on two systems of rocks, one—the basal system—having been folded or sheared at a former time and since denuded, the upper series being sedimentary beds laid down upon the basal series, enables us to understand the differential effect of simultaneous pressure on two such systems. The basal series become more compressed and distorted, and this, if the compression were continued, would be likely to end in fracture and shear-planes. These thrusts are eminently characteristic of rigid strata that

[1] P. 40 D.

have suffered from intense pressure. The upper series are not rigidly bound, and the pressure expends itself in folding. As already pointed out, we must discount the effect of the domical rise which in our models leaves vacuities between the individual domes, for in nature gravitation would keep the strata practically together as solids.

In a memoir of the Geological Survey of India on the Physical Geology of the Sub-Himalaya of Garhwal and Kumaun, C. S. Middlemiss gives a number of interesting sections across the sub-Himalyan zone in which the hade and succession of reversed faults seem to point to the dominant pressure having acted from the north.[1] The crescentic plan of this mountain system, with its concavity to the north, would seem to favour the same conclusion. How far the pre-Tertiary beds were folded before the uplift of the Himalayan system is too complex a question to enter upon here. Divergence of strike in the folded regions no doubt points to pre-Tertiary compression, but I must maintain that the system as a whole is Tertiary. As Dr. R. D. Oldham penetratingly observes, 'the occurrence of marine nummulitic beds at a height of many thousand feet on the north face of the main snowy range in Hundes, and at a height of 20,000 feet in Zanskar, shows that the elevation of this part of the Himalayas

[1] Suess is of opinion that there has been in Asia a general movement of rock-masses towards the south. For a summary of his views see *History of Geology*, by Von Zittel, translated by Maria Ogilvie Gordon, D.Sc.

must have taken place entirely within the Tertiary period.'[1]

Mr. Middlemiss has done me the honour to devote several pages of his memoir to a discussion and attempted refutation of my theory of mountain building. He cannot accept, for one thing, the central fact upon which it is founded, viz. the connection of mountain building with previous sedimentation. This is disputed by very few geologists. Hall, Dana, and Le Conte had pointed it out before my time, though they failed to see the true relation as cause and effect. I do not propose to go into details here, but will merely observe that Mr. Middlemiss, when he thinks he is 'beating a corpse,' in some curious way misunderstands the physical question involved.

With these remarks I must end this chapter. My desire is not to enter into controversy, but to state my views clearly, so that geologists may be in a position to test their validity. I know full well that we can only hope to arrive at partial truth.

What there is of value in these pages will be separated and preserved by the impartial hand of time. As observations are multiplied and recorded future geologists will possess a far broader basis of facts from which to draw their conclusions. The subject is so new and the innumerable relations so complex that it will doubtless be long before anything like unanimity is arrived at.

[1] *A Manual of the Geology of India*, second edition, p. 477.

CHAPTER XVII

INSTANCES OF THE EFFECT OF EXPANSION DUE TO ATMOSPHERIC CHANGES OF TEMPERATURE

EXPERIENCE proves that most important changes are going on around us of which we perceive ordinarily no indications.

The linear dimensions and volume of every substance are in a continual state of flux and reflux with every change of temperature. It is only when these movements affect us personally that we pay much attention to them. I have collected a series of examples of the way atmospheric changes of temperature may interfere with our conveniences. Engineers have to pay special attention to these points, as the success or failure of their works is largely dependent upon the provisions which are made to meet them.

The instances I am about to quote are in continuation of Chapter III. of 'The Origin of Mountain Ranges,' and will help the reader to realise what an important part variations of temperature must play in the reactions of the heated interior globe upon the ever, but slowly, changing lithosphere.

Expansion of Streets—Lifting of Street Paving.—'A street in Terre Haute, Indiana, had been paved with brick five years ago, the joints being grouted up. The work was partly done during the winter and was finished in early spring. The foundation was of broken stone, 8 inches thick, above which was a layer of sand 2 inches thick. At the end of last July—thermometer at 100° F.—a section of the pavement rose like an arch from its foundation, and though water was turned on to it and openings made to let out any possible accumulation of gas, it maintained its position unaffected. While this was being repaired another section rose in a similar manner, with a loud report, to a height of 7 to 8 inches.'[1]

Effect of Hot Days on Concrete and Asphalte Footwalks and Carriage-ways.—Mr. C. R. Strachan, C.E., says: 'It is no uncommon thing to see the surface of an asphalte path raised crosswise in an irregular line, as though a small tree-root were under it. In every case where the asphalte has been uncovered at these points by the writer he has found the concrete crusted.' This effect is most marked on hot days. In some laid by him in Chelsea the number of these ridges in the hot weather of June was most astonishing. 'Shortly after midday they were most pronounced, and towards night they were less prominent.' He mentions kerbs in the City as being displaced from the perpendicular by pressure of the concrete of

[1] *Nature*, September 3, 1896, p. 425, quoted from *Engineering*.

the carriage-way and footway, also due to the sun's rays.[1]

Linear Expansion.—The effect of hot weather upon steel rails is well known, for hardly a summer passes without some inconvenience to railway traffic being recorded through the buckling of the rail tracks. The following are a few among innumerable instances:—

Trains blocked at Prescot, Lancashire, on the London and North-Western Railway.—'So intense was the heat (June 13, 1887) in the afternoon that some of the rails between Huyton and Prescot became twisted, and for a time put an entire stop to the whole of the traffic between Liverpool and the stations going North.'[2]

A well-known railway engineer (chief) writes to the 'Engineer' (July 15, 1887, p. 49): 'I am very busy with the line this hot weather; in 25 miles 42 rails have had to be cut off 4 inches—that is, 14 feet in all—and afterwards as equably divided as possible. One of my best inspectors declares that steel rails grow.'[3]

A Creep of Rails.—'A permanent-way engineer says that in consequence of introducing fishbolts that require to be screwed up very tight 300 yards of railway were rendered impassable by buckling; now the fishplates are loosened he is threatened

[1] *Builder*, July 30, 1887, p. 156.

[2] *Liverpool Daily Post*, June 14, 1887.

[3] It is extremely probable that the rails may suffer permanent extension, as we see lead, and even concrete, does.

with a creeping line that will jam up the joints at the bottom of the hill.'[1]

Rails in India and Australia.—Great difficulties have been experienced in these countries from the rails buckling in hot weather. Spring washers are used to give the rails play and allow for expansion.[2]

Permanent Lengthening.—In the 'Geological Magazine,' January 1888, p. 26, I have described the effect of alternations of temperature on terra-cotta copings set in cement. These copings are on fence walls enclosing the building plots, and are exposed upon all except the undersides to the vicissitudes of the weather at Blundellsands. There are many examples of permanent lengthening of the copings, for which I can find no other explanation than the effect of the sun's rays. Where what is technically called a ramp—that is, a curved portion of the coping connecting two different levels—occurs, the effect of the lengthening is plainly seen by a lifting of the upper part of the ramp, and often a considerable length of the coping. Mr. John D. Paul, F.G.S., writes to say he has observed the phenomena in many places in Leicester, and kindly sends me a photograph of the ramp of a coping similarly affected to those I have described at Blundellsands. Mr. Paul independently gives the same explanation that I do.

I have since made a good many observations, and find that appreciable lengthening takes place in spells of very hot weather. The curious thing

[1] *Engineer*, July 1, 1887, p. 9. [2] *Ibid.*, April 6, 1888, p. 275.

is that in some cases the 'creep' goes on after the coping has been taken off and reset several times. The phenomenon, though common, is not universal, and I know of one case of a long wall where the force of the expanding coping has been so great at one end as to fracture the wall below, while the other end is apparently unaffected. In another case the expansion has been so regularly distributed that it is *nil* at the centre and goes on increasing towards either end, being prettily shown by the shearing of the mortar bed of the corbel bricks where the coping rests upon them.

Some of these copings I have watched for years since they were first set, and have followed the initiation and gradual lifting of the coping at the head of the ramp. In one case it was lifted off its bed $1\frac{5}{8}$ inch vertically before the proprietor had it reset.

I have also observed this peculiar effect in other localities, but only in copings or analogous structures specially exposed to the sun's rays. I have never, in all my experience, seen a wall built in cement or mortar that showed any signs of having been subjected to such movements.

Contraction of Cement and Cement Concrete.—To settle the question whether or no the cement joints had expanded, and so caused the lengthening, I made the following experiment:—

A bar $1\frac{1}{2}$ inch square in section was made of the very best Portland cement and sand in the proportion of 1 to 1. The ends were enlarged

and perforated, with the object, if necessary, of putting a tensile strain upon the bar by suspension and hanging weights thereto. In the bar were imbedded brass studs for the purpose of measuring by special callipers any alteration of length that occurred. The distance between the studs was 16·136 inches when the bar was three weeks old. This was on September 1, 1887. A record was kept from time to time of variations of length. The bar was, during the greater part of the time, up to 1901, kept in my cabinet at a fairly equal temperature, but a part of the time it was exposed at a south window, and afterwards in a glass porch, but always under cover. Under the latter conditions the occurrence of a sunny day produced its effect in an appreciable expansion, but on the whole there was a steady decrease of length for the first twenty weeks. After this, up to 1900, it still shrank, but in a gradually decreasing ratio. To make these movements plain to the eye Mr. M. Treleaven Reade constructed the diagram, Plate XXXA., showing the contraction that would take place if the bar were 100 feet long and under similar conditions.[1] During the time the bar was in the glass porch, exposed to the sun's rays, it was suspended by one end to a bracket and loaded at the other up to a tensile stress of about 40 lb. The total contraction for 100 feet = 1·86 inch in 13¼ years.

It is quite evident from these results that the

[1] The actual shrinkage can be directly measured at any point of time on the diagram, as it is shown full size.

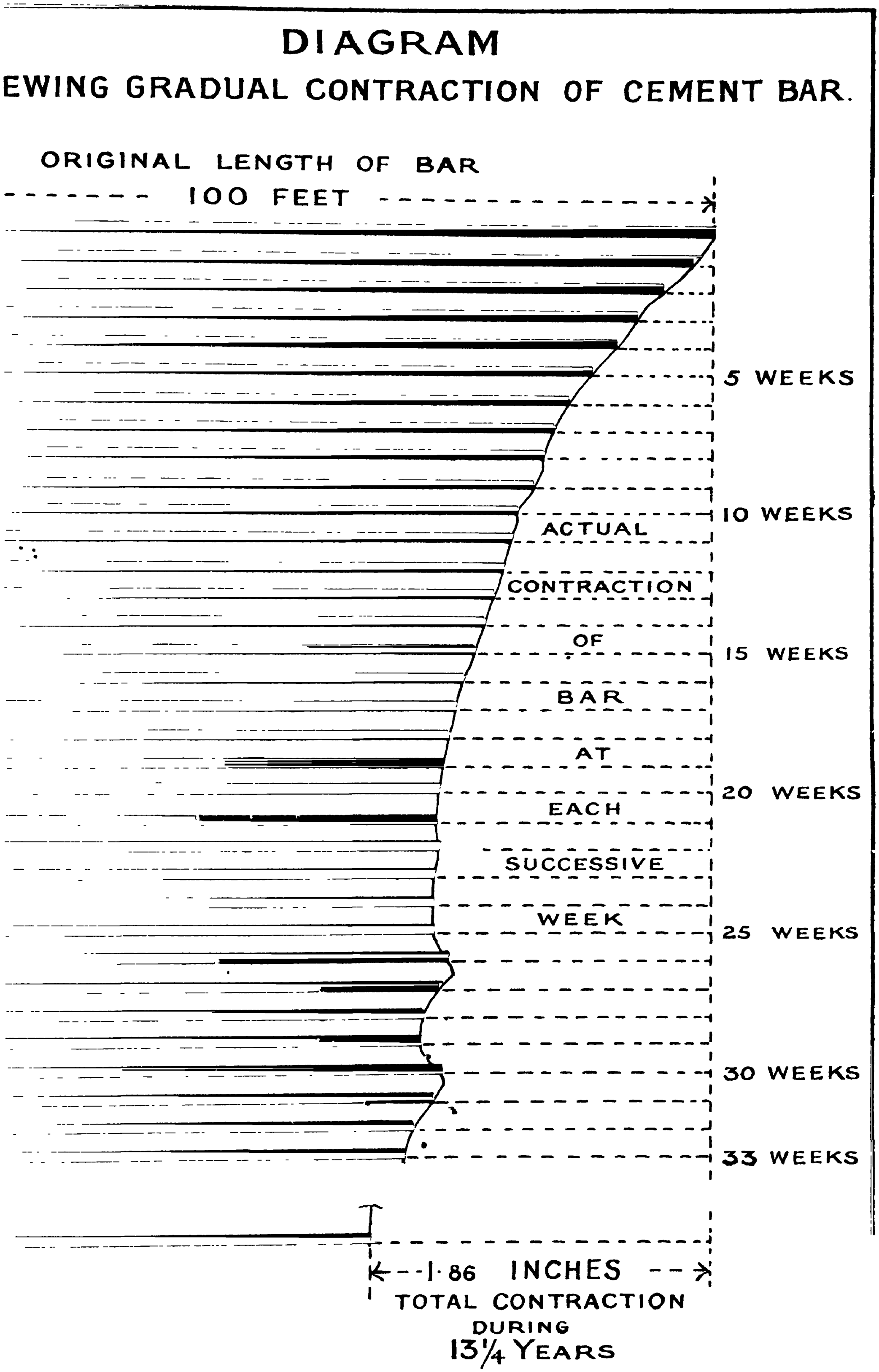


DIAGRAM
EWING GRADUAL CONTRACTION OF CEMENT BAR.
ORIGINAL LENGTH OF BAR
100 FEET
5 WEEKS
10 WEEKS
15 WEEKS
20 WEEKS
25 WEEKS
30 WEEKS
33 WEEKS
ACTUAL
CONTRACTION
OF
BAR
AT
EACH
SUCCESSIVE
WEEK
1·86 INCHES
TOTAL CONTRACTION
DURING
13¼ YEARS

lengthening of the copings is not due to expansion of the cement joints.

The coefficient of expansion was found to be $\frac{1}{142156}$ of its length per degree Fahr., and it would require to be heated to about 290° Fahr. to bring the bar back to its original length of 16·136 inches.

The New York Dock Department found that their long concrete walls contracted $\frac{3}{8}$ of an inch per 100 feet.[1]

Other Effects of Expansion.—The oft-repeated stresses caused by variations in temperature in climates where the daily range is great have a powerful effect in disintegrating the rocks.

'Keane and other Arabian travellers have described how, on a cold night following a hot day, rocks are often found loudly cracking and splitting up. At 15,000 feet, in the Himalayas, the difference of temperature of day and night is as much as 80°, and this breaks up the rocks with extreme rapidity and provides a mass of loosened *débris* never found in Europe.[2] Professor J. C. Branner, in an interesting treatise on the 'Decomposition of Rocks in Brazil,'[3] describes how severe changes of temperature in combination with other agencies have produced characteristic topographic rounded forms, or bosses, and conical peaks in the gneissic rock-masses of Brazil. In some cases exfoliation has gone on so energetically that the 'surface has

[1] W. R. Hutton, *British Architect*, December 1, 1893.

[2] *Climbing in the Himalayas*, Sir Martin Conway.

[3] *Bulletin Geo. Soc. of America*, vol. vii. pp. 255–314; see also *Geo. Mag.*, March 1897.

flaked off in great sheets that have slid down the mountain slopes.'

Teall, in his interesting but all too short Presidential Address to the Geological Society of London (1902), speaking of 'dry weathering,' which goes on in desert regions and arid climates of closed drainage, says: 'The violent extremes of temperature not only detach fragments from every exposed surface, but often loosen the constituent crystalline rocks, so that at the slightest touch a piece of apparently solid granite will crumble into sand, leaving the felspars as fresh as when they formed a part of the original rock.'

EFFECT OF DIURNAL AND WEEKLY CHANGES OF TEMPERATURE ON THE ICE OF THE INLAND LAKES OF WISCONSIN 'ICE RAMPARTS'

One of the most interesting natural results of changes of temperature is that described by Buckley as affecting the ice which in winter covers the inland lakes of Wisconsin. 'During the winter of 1898–99 the precipitation and temperature conditions in South Central Wisconsin were exceptionally favourable to the formation of ice ramparts by the moderate-sized lakes.'[1]

These 'ramparts' are ridges of sand, gravel, and ice pushed up the banks of the lake by the expanding ice sheet during a rise of temperature; they are usually parallel to the shore. At Picnic Point, Lake Monona, where there are vertical banks of

[1] 'Ice Ramparts' (*Trans. of the Wisconsin Academy of Sciences*, vol. xiii. pp. 141–62).

clay, the ice pushed up a ridge having an average height of about 4 feet, a breadth of base about 11 feet, and a breadth at the top of about 4 feet. At one place the bank was not less than 8 feet high, and carried on the top a tree of considerable size. Elsewhere trees of over 12 inches diameter were dislodged and overturned. Another kind of ice rampart has the shape of more or less symmetrical folds, which always occur near the shore where the water is shallow. Under certain conditions a single fold is formed; in others, a series of consecutive folds resembling the Appalachian mountain structure. In another case the expansion of the ice apparently resolved itself into two components, acting approximately at right angles to each other, making the longer axes of the folds on the two sides of the bay nearly normal to each other.

Mr. Buckley goes on to explain that the upper surface of the ice is usually at about the temperature of the air; the under side, that of the water. The upper surface is, therefore, the coldest.

A fall in temperature produces a contraction of the upper surface of the ice, which gradually decreases towards the under surface. Under these stresses the ice begins to rupture at the surface, till in some cases the crack reaches the water, which wells into it and freezes solid. If a sudden rise of temperature should now occur, the conditions are reversed, the upper surface of the ice expanding and tending to rupture the sheet of ice on the under side. The recurrence of these movements

through rapid variations of temperature enlarges the area of the ice; hence it relieves itself by encroaching on the land, and the phenomena of ice ramparts and folds result.

These changes are analogous in their history to those already described in Chapters IX. and X. (Plates III., IV., V., and VI.) as occurring in the lead sheet. Taking into consideration the difference in the physical properties of ice and lead, the similarity is striking.

It seems to me that the cracking and freezing up are not quite such important elements in the expansion of the sheet as at first sight would appear. We have seen that even such an intractable material as terra-cotta is influenced by changes of temperature in much the same way without cracking. Ice is to a certain extent plastic like lead, from whatever cause it may arise.

The similarity of these phenomena to those of mountain building is striking, and is dwelt upon by Buckley. In the discussion following the paper Professor Van Hise emphasised the analogies between these movements and those that give birth to mountain structure by folding, but is not prepared to draw the natural inference that they are due to the same causes. I trust it may not be thought presumptuous if I commend the study of these phenomena as an admirable object-lesson illustrating my theory of the origin of mountain ranges.[1]

[1] G. K. Gilbert was, I believe, the first to explain the cause of ice ramparts in his monograph on Lake Bonneville.

CHAPTER XVIII

NORMAL OR CONTRACTION FAULTS

WE have now experimentally analysed the component movements imparted to strata by lateral compression applied in several ways and under varied conditions. Furthermore, it has been shown that the internal stresses set up in the lithosphere by alternate expansions and contractions would result in movements of the rocks comparable to what we see in mountain chains and folded regions of the earth's crust.

It has also been pointed out, both here and in 'The Origin of Mountain Ranges,' that a secular contraction of the earth's nucleus, on the contractional hypothesis, provides only continued or successive *compressive* stresses in the lithosphere, *but no alternations with tensile stresses*, such as are registered by normal faults in almost every accessible section of the crust of the earth.

This, I conceive, is the inherent weakness of the contractional hypothesis. Suess, in his important work, 'The Face of the Earth,' clearly recognises the existence of two types of structure in the earth's crust. As admirably summarised by Mr. Teall, he holds that 'a study of the structure

of the earth's crust proves that it has been affected by two types of movement—the one characterised by a compression of the stratified rocks along certain zones; the other, by a separation of the crust into blocks, some of which have sunk down relatively to others;' and he goes on to say: 'One and the same area cannot be simultaneously affected by these two types of movement.'[1]

Though these two types of structure are recognised, Suess apparently does not see their true relations; nor is it explained how this striking difference of effect should be produced by the same cause, for Suess seems only to recognise radial contraction in his theory of the earth. In 'The Origin of Mountain Ranges' I have attempted to show that the *folding* is due to lateral expansion, caused by a rise of temperature acting either continuously or successively on a compound aggregate of strata, termed by me the 'strata-plate.' The best conception of this action is to be gained by a study of Plates III., IV., V., and VI. of the present work. Normal faulting, *by which blocks of the crust are let down*, relatively to others, is the result of *contraction* following a fall of temperature. The *folding* is due to compression, the *faulting* to tension. Since 'The Origin of Mountain Ranges' was published normal faults have been pretty generally recognised as due to *tension*. This necessary complementary and opposite stress to

[1] *Nature*, December 19, 1901, p. 145.

that of compression is, so far as I am able to discover, unprovided for by any theory of mountain building other than my own.

The contraction hypothesis, as I have reiterated —but apparently none too often—only provides a continuously compressive force. Normal faulting results from the shortening of the strata, together with the inadequacy of the crust to maintain voids. If the materials of the crust were of sufficient rigidity and strength, the cubical contraction due to a fall of temperature would simply cause great rifts, which would remain open until filled up with *débris*, and the surface of the blocks, though sinking actually, would not be differentially lowered with respect to each other. The rigidity and tenacity of the materials of the crust are, however, but slight compared with *the enormous forces simple gravitation lets loose.* The yielding crust sinks differentially along huge shearing planes called faults which it is well known 'hade to the downthrow.' The horizontal contraction is thus provided for by the keying up of the strata-plate in sections, which enables it to occupy a larger horizontal area than it otherwise would, and at the same time keeps the crust solid and continuous. Nor must it be lost sight of that a fall of temperature, within certain limits, increases from zero at the surface to a maximum at a certain depth, which depth is determined by several factors. The combined result is, that below a certain depth the normal

faults may shade off and the vacuities—or rather what would otherwise be vacuities—in the crust become filled solid by a flow of rock rather than by keying up. The effects of all these factors in filling up tri-dimensional space, and maintaining a solid crust, are to be seen in the mapping of any well tension-faulted area. A secondary effect is often the turning up of the edges of the strata against a boundary fault, through the compression produced by the blocks keying up at the upper part before the mass comes to rest on a solid foundation.

The fact, well known to engineers, that faults are often the most watertight parts of the strata, is another proof, if one were needed, that contractional faulting does not produce an open fissured crust, but that the keying by the sheared and sinking blocks goes on simultaneously with the sinking, and that enough lateral pressure is so produced by the gravitating mass to maintain a solid continuous lithosphere. There is every reason to infer, as geologists usually do, that the whole of these adaptations take place very slowly, whether accumulating stresses be relieved by small or large successive slips. That the forces to which these two broad types of structure—the folded and the normal faulted—are due cannot be simultaneously operative in the same area is very obvious, the one being the antithesis of the other. I have shown that no theory which does not provide for *both* movements can be a true explanation of the

phenomena of the earth's crust. Perhaps my insistence on these points will be pardoned as, so far, the difficulty appears to be unnoticed by those who think that the leading dynamical phenomena of the earth's crust are altogether due to its settling down upon a shrinking nucleus.

CHAPTER XIX

SLATY-CLEAVAGE

THE experiments which have been detailed in the previous chapter prove that under compressive stresses the beds or strata-plates—miniature representations of portions of the earth's lithosphere—undergo complex movements, and are bent, twisted, thickened, and even stretched in the process. In adapting themselves to these new conditions the materials of which the beds are composed flow and shear in directions governed by the thickness or thinness of the plates, the direction and strength of the compressing forces, and the plasticity or rigidity of the materials. In Nature there is a vastly greater variety of material in assembled series than are represented in our models. There may be fine clay, granular sedimentary deposits, conglomerates, volcanic beds, igneous dykes and sills, or the wonderful combination of compressed granitic, felsitic, and other intrusive igneous rocks usually constituting an Archæan complex.

When the compressing forces which affect the earth's crust from time to time act upon these heterogeneous beds, the variety of movement and

resulting forms must of necessity be greater than in our clay models.

Instances of parallelisms of bedding planes over large areas that have been subject to great compression can, however, be pointed to. The South of Ireland is an example, which will presently be dealt with.

Parallel Structure.—The movements illustrated in the clay models subjected to compression are, mainly, folding and shearing of a more or less complicated and often convoluted type, and it has been shown that this involves flow of material from one place to another in diverse rather than parallel directions.

The structure known to geologists as slaty-cleavage is eminently a parallel structure, which cuts through folds, convolutions, and even twists in strata with such remarkable indifference that the normal bedding or planes on which the rock was originally laid down become practically obliterated as a structure. The rock no longer splits along its bedding planes, but along the induced parallel structure, which ignores the original conditions of deposition. There is, however, one great fact, often lost sight of, namely, that all classes of rocks cannot develop slaty-cleavage, however much they may be compressed or subjected to identical influences.

It is here that the mechanical theory, which attributes slaty-cleavage to pressure, and pressure alone, fails.

Why this should be will form one of the aims of our inquiry.

Investigations of Slaty-cleavage

The subject of slaty-cleavage had long possessed great attractions for me, and after the publication of 'The Origin of Mountain Ranges' I further studied it in the field and collected typical specimens.

It was not, however, till 1897 that the investigations about to be described were undertaken in collaboration with Mr. Philip Holland, F.I.C.

The first result of our work was the publication of a paper on the 'Phyllades of the Ardennes compared with the Slates of North Wales,' Part I.,[1] followed by Part II. in 1900,[2] and in 1901 by 'The Green Slates of the Lake District.' In the two latter we were kindly assisted in the microscopical work by Mr. Maynard Hutchings, to whom we wish to express our indebtedness.

In the last of these papers we sketched out a 'Theory of Slate Structure and Slaty-cleavage,'[3] founded upon our investigations, and as it in substance contains our present views I have thought it simpler and better to reproduce it verbatim than to recast it specially for this work.

Since the publication of this theory we have carried on further investigations, the substance of which will be found recorded in the ensuing pages.

For the sake of completeness, and to add to the

[1] *Proceedings of Liverpool Geological Society*, 1898, pp. 274–93.
[2] *Ibid.*, 1900, pp. 463–78. [3] *Ibid.*, 1901, pp. 101–27.

practical value of the work, I reproduce at the end of this chapter the whole of the tables of chemical analysis published in the papers to which I have already referred.

A Theory of Slate Structure and Slaty-cleavage by Reade and Holland[1]

The Chemical and Mineralogical Make-up of Slates.—It will be noticed on careful examination that, in all the examples of slates analysed by us and reported upon mineralogically by Mr. Hutchings, slaty-cleavage and the development of micaceous and chloritic minerals appear to be indissolubly associated. These minerals have been 'rolled out' in the direction of the cleavage planes. They are evidently secondary, and we cannot but think there is good evidence to show that they have been developed *pari passu* with the movement of the constituent particles of the rock which has given way under shearing stresses. High temperature, pressure, and movement acting upon a rock-base chemically and mechanically fitted for the manufacture of the slate, if we may be permitted to use that expression, seem to be a necessary conjunction for the production of slaty-cleavage, and the more foliaceous the constituent particles, and the more abundantly they are present, the firmer and more perfect will be the cleavage.

[1] From *Proceedings of Liverpool Geological Society*. 1901, pp. 120 27.

The rougher slates are made up of larger clastic or crystal grains or particles, and the whole aggregate, whether large or small, is made intimately coherent by the presence of secondary minerals of a foliaceous character, which act not only as structural parts of the slate and longitudinal ties, but also as a cement.

We have already pointed out [1] that, in our view, the slaty-cleavage has been impressed upon the rock in a late stage of its individual history. In confirmation of this we may point to the extraordinary distortions the banded rocks of Tilberthwaite and Buttermere have undergone [2]—a distortion that, had it occurred after the cleavage structure was developed, would have affected the cleavage planes themselves.

This has not occurred, and it is this great fact of the cleavage planes in all slate rocks traversing the rock in parallel planes, quite independently of the bedding, that inclined the great Sedgwick, who was the first to study the phenomena scientifically, to think that cleavage was a sort of crystallisation on a large scale, or due, as he expressed it, 'to crystalline or polar forces acting on the whole mass simultaneously in given directions and with adequate power.' [3]

Old and New Views and Theories of Slaty-cleavage tested and compared.—This short sum-

[1] Part II., 'Phyllades,' &c., p. 476. [2] Pages 107 and 110.

[3] 'On the Structure of Large Mineral Masses' (*Trans. Geo. Soc.* vol. iii., 1831).

mary of our views on slaty-cleavage, founded entirely upon observation and experiment, may, with profit, be compared with the views that have been put forward from time to time by prominent geologists and physicists. It has been a favourite subject of speculation with *savants*, as it possesses the interest which always attends the mysterious and difficult. Sorby, who was the first to study the microscopic structure of slate, sums up his conclusions thus: 'What I chiefly wish to impress on the attention of geologists and physicists is that slaty-cleavage is due to mechanical causes; that cleaved rocks are compressed rocks; and that the compression in general has not only changed the arrangement of the unequiaxed particles of which they are composed, but in some cases has altered their form. There are scarcely any rocks whose particles are not thus unequiaxed, and I still maintain that, other circumstances being the same, those have the best cleavage that are composed of particles whose length and thickness differ most.' [1]

Tyndall, on the contrary, considered that slaty-cleavage is independent of the shape of the con-

[1] 'On the Theory of the Origin of Slaty-cleavage' (*Phil. Mag.*, August 1856).

In his Presidential Address to the Geological Society of London on 'The Structure and Origin of Non-calcareous Rocks' (*Proc. Geol. Soc.*, 1879-80), Sorby returns to the subject of slaty-cleavage, and discusses further details, the result of wide investigations; but he still maintains substantially the same views, and says: 'The mechanical theory of cleavage is now so generally accepted that it may perhaps be thought superfluous to support it by further facts.'

stituent particles, and that simple compression is competent to produce cleavage in the finest material, instancing his own experiments in the compression of wax. In fact he held, the finer the material, the more perfect the cleavage. Tyndall is sceptical as to the importance of the presence of mica, and says in a criticism of Sorby's views: 'I cannot accept his explanation of slate-cleavage. I believe that even if these plates of mica were wholly absent the cleavage of slate rocks would be much the same as it is at present.' [1]

These three views of Sedgwick, Sorby, and Tyndall on slaty-cleavage express the salient differences in the way of looking at and explaining the phenomena.

There is a fourth view, put forward in 1875 by Dr. Wm. King, in which slaty-cleavage is attributed to the compression of parallel joint planes,[2] a view apparently supported by Becker.[3]

Sharp, Phillips, Darwin, Haughton, and others have contributed to the discussion and elucidation of several phases of the phenomena; and, among the earliest geologists, Bakewell considered the structure to be 'the effect of crystallisation,' while MacCulloch supposed it to be 'the result of concretionary action.' It is unnecessary for us to

[1] *Phil. Mag.*, 1856. p. 44.

[2] 'On Jointing and its Relation to Slaty-cleavage' (*Trans. of Royal Irish Academy*, vol. xxv., 1875).

[3] 'Finite Homogeneous Strain, Flow and Rupture of Rocks' (*Bul. Geo. Soc. America*, vol. iv. p. 75) contains a discussion of theories of slaty-cleavage.

deal with the history of the subject here, as Mr. Alfred Harker has thoroughly expounded the various leading theories in the British Association Report, 1885, 'On Slaty-cleavage and Allied Rock-structure, with special reference to the Mechanical Theories of their Origin,' and Prof. John Phillips previously, in 1857, reported to the British Association 'On Cleavage and Foliation in Rocks, and on the Theoretical Explanations of their Phenomena' (p. 369).[1]

Examination of Sorby's and Tyndall's Theories, and Exposition of our own Conclusions.—The investigations we have undertaken, which form the subject of these papers, have been principally confined to chemical composition and microscopic structure. The larger field-work, or geological aspects, have only entered into our purview as incidental matter, but they have not been unconsidered.

In the light of the facts set forth in this paper let us examine briefly the purely mechanical theory of the origin of slaty-cleavage.

Tyndall stands up for it in the most uncompromising form, and considers that compression

[1] One of the latest investigators—Mr. T. Nelson Dale—in a memoir entitled 'The Slate Belt of Eastern New York and Western Vermont,' which contains a vast amount of information, says: 'Perhaps slaty-cleavage may be defined simply as a rearrangement of the particles of a deposit by pressure, and a simultaneous arrangement of any new crystalline particles formed during that pressure. This arrangement of old and new particles is related to the directions of pressure and resistance' (*U. S. Government Survey, Nineteenth Annual Report*, 1897 98, Part III. 'Economic Geology,' p. 205).

will induce this structure on practically any fine material.

Tyndall was not a geologist, or he would have known that many rocks which seem mechanically fitted to have cleavage structure impressed upon them, and which show abundant evidence of having been subjected to great lateral pressure, present not the slightest trace of slaty-cleavage, such, for instance, as the much-folded rocks, largely greywackes, of the South of Scotland, which are finely exhibited along the Berwickshire coast.

Tyndall alleges: 'When slate mud is compressed its different layers cannot be expected to move laterally with exactly the same velocity; they slide over each other, and this action, as in the case of iron, must also tend to weaken the cohesion of the layers and to facilitate the cleavage of the mass.'[1] This is not compression, but shearing, and it appears to us that such shearing would be most effective in rocks composed largely of particles having a predominant long axis. There is not, however, the slightest evidence that slaty-cleavage was imposed upon rocks when in the state of mud; nay, the opposite is the view we hold, viz.: that slaty-cleavage was superinduced upon the sediments after they were tightly folded and consolidated—when lateral pressure could act as on a homogeneous material. Even if we admit Tyndall's view of the effect of shearing upon muds, it is quite clear that such a rearrangement of particles would

[1] Observations on Sorby's paper already quoted.

not weld them together so that the cleavage planes should cross and obliterate the bedding planes, and give to slate that transverse strength which so distinguishes it from most rocks.[1]

Sorby, we believe rightly, held that the slaty-cleavage is influenced by, and greatly due to, the shape of the constituent grains, but we cannot agree with him, or with Tyndall, that the phenomena are wholly mechanical, or that simple compression would produce the parallelism necessary for the production of slaty-cleavage. In fact, there is good reason to believe that little actual condensation (*i.e.* reduction of volume) took place during the

[1] By experiment one of us found, many years ago, that a Velenhelli slate, cut along the *dip plane*, having a transverse section of 3 inches by 0·187 inch, supported on bearings 18 inches apart, broke with 29 lb. 11 oz. applied at the centre = to a breaking weight of 283 lb. for a bar 1 inch square and 18-inch bearings. The strength of such a bar cut *transverse* to the dip plane was found to be only half that.

We can find no record of tests of the transverse strength of slates, but Kinnear Clark gives the strength of English oak beams, 2 inches square and 6 feet bearing, at from 407 to 813 lb. breaking weight. A slate beam of the same dimensions, having a coefficient of transverse strength equal to the Velenhelli slate in our example, would break at 560 lb.

In a Memoir on 'The Building and Ornamental Stones of Wisconsin,' by E. R. Buckley (*Wisconsin Geological and Natural History Survey*, 1898, Bul. No. IV. Economic Series, No. 2, p. 396), a table of the transverse strength of Wisconsin building-stones is given. The experiments were made on bars 1 inch square in section and 4 inches bearing, the weight being applied in the centre. The Montello granite, the strongest stone tested, in the specimen yielding the highest result broke with a load of 794 lb. This would be equal to 336 lb. for a bar 2 inches square in section and 6 feet bearing, as compared with 560 lb. in our experiment with Velenhelli slate. The limestones varied very much in transverse strength, but were distinctly weaker than the granites. The sandstones varied still more, some being exceedingly weak, and the strongest less than one-third the strength of the strongest granite.

development of the cleavage, the specific gravity of the rock having been, we believe, as great or nearly as great immediately before as after the rock became *cleaved. The reduction of volume of the mud preceded the development of cleavage; and if this* were so the movements of the fragments of the rock would be entirely shearing movements—a change of form without change of volume.[1]

Our remarks have already extended to a greater length than originally contemplated, but we shall have quite failed in our object if we have not made it plain that chemical action bringing about mineralogical change is an important factor in the production of slaty-cleavage.[2]

To summarise what has already been said, mechanical pressure, compression, and shearing may in some cases produce a cleavage structure in rocks. But these agencies alone cannot cause true

[1] The specific gravities of the Lake District slates, it will be seen on reference to the Table, are wonderfully uniform; they are less than the Welsh slates and phyllades given in Part I., 'Phyllades of the Ardennes,' but they, again, have little variation among themselves. Further condensation would not result from pressure, but change of form by shearing. The shearing planes would not necessarily be developed at right angles to the pressure; indeed, we have seen slate quarries in which the cleavage dip was at so low an angle that this could not have happened.

[2] Since this was written we find that Van Hise has expressed himself as follows: 'My microscopical studies of both cleavable slates and schists have convinced me that in the interstices, and by the decomposition of the larger particles, new minerals, and especially mica, abundantly develop, with similar orientation, and with their larger diameters, or cleavage, or both, parallel to the flattened or rotated original particles' (*Sixteenth Annual Report of the U. S. Geological Survey*, 1894-95: 'Principles of North American Pre-Cambrian Geology,' p. 635).

slaty-cleavage, which we believe to be a new physical condition indissolubly associated with the built-up platy-structure we have described in detail. This structure we have likened to the bond of stone in a wall further intimately tied and bonded together by platy minerals, the whole being cemented into a coherent rock by secondary minerals developed during the continuance of the shearing forces. That shearing forces were in operation is sufficiently indicated by that rolled-out appearance of the minerals to which Mr. Hutchings so often alludes in his description of the slides. The minerals seemingly develop along planes of movement, and a little consideration will show that parallel planes could not result from external pressure unless the force acted on a homogeneous material; hence we may legitimately infer that the beds of rock, which often show so much disturbance and folding, have been packed and pressed into a homogeneous mass before the pressure, heat, and chemical action which produced the cleavage planes and slaty-cleavage structure had acted upon the rock.

Finally, when we consider the enormous transverse strength of slate compared with other building-stones, a strength which our experiment in 1889, already described, showed was, in the case of a Velenhelli Welsh slate, equal to that of good English oak, we cannot fail to see how inadequate mechanical pressure alone would be to produce from a mud a structure possessing such remarkable

qualities as slate. We have, we hope, clearly shown that real slaty-cleavage is always accompanied by mineral changes in the body of the rock, which not only give to the rock its foliaceous character, but supply the necessary cement to bind together the heterogeneous overlapping constituent grains, and convert what was originally mud into a rock possessing the tenacious and economically useful properties of slate.

The Old Red Sandstone and Carboniferous Slate of the South of Ireland

For the completion of our studies of cleavage it was thought advisable to visit the South of Ireland, where the vast series of rocks coeval with our Old Red Sandstone and Carboniferous formations have suffered intense lateral pressure.

This pressure has been so uniformly applied in one direction, or in opposite but parallel directions, that the strike of the rocks throughout the part of County Cork visited is practically identical. The rocks are mostly thin bedded, and the dip is either vertical or approaching it. At the first sight of one of the quarries near Clonakilty Lough, on the west side, it was found difficult to say whether the structure exhibited was due to cleavage or bedding.

A closer examination showed that it was undoubtedly bedding, but probably affected by the lateral pressure that had lifted and folded the

rocks. The structure was a parallel one, but parallel to the bedding. The proof of the planes being those of bedding lay in the fact that the nature of the rock varied from plane to plane, or from one group of planes to those of another group. In one it might be a fine fissile shale, in the other a fine grit.

These changes will be noted in detail as the specimens are described. The fissility is in the plane of bedding, but in some cases there seemed to be developed a cleavage at a very low angle to the bedding, so that the split surface had a minute ripple-marked appearance.

The dip of the beds was 80° north by west, the strike easterly and westerly, with a deflection to the north-east. Specimens 9, 10, and 11 came from this quarry, which is at a point near to the junction of the Upper and Lower Old Red Sandstone. Anything less like one's lithological ideal of 'Old Red' it would be difficult to find.

This class of rock, though possessing a fine fissile structure, cannot be said to possess slaty-cleavage. Though, as the analyses show, the mineral constituents are analogous to those of true slates, the rock is mostly very soft. It is used in the neighbourhood for rough walling.

Specimen No. 3 (Quarry No. 2), from the east side of Clonakilty Lough, though in structure and composition a true slate, is not a roofing slate. The junction of this rock with the adjoining rocks could not be seen, but No. 4 (Quarry No. 1) is a

gritty fissile rock, splitting along the bedding plane.

Carboniferous Roofing Slate.—At Benduff, about 2 miles north-west of Rosscarberry, is a considerable slate quarry in the Carboniferous Slate system. Mr. Swanston, the proprietor, kindly sent his foreman round the quarry with one of us. It is a glossy black slate with an excellent cleavage, and most of the buildings in the neighbouring towns appear to be roofed with it, and much is exported. The miners recognise the following structural peculiarities in the rock: 1st, cleavage; 2nd, heads; 3rd, joints; and 4th, floor. The 'heads' appeared to me to be joints that have become partially open so as to separate in the quarrying.

The 'floor' is, according to my interpretation, a more marked plane in the original bedding which has become corrugated by the lateral pressure. The bedding structure is more or less in evidence in most of the slates, but the 'floor' represents a more pronounced difference in the nature of the deposits—such a difference as to cause a break in the continuity of the cleavage. Another peculiarity, which, however, is not of such frequent occurrence as to affect the value of the slates, is what the quarrymen term 'bulls,' which are small lenticular swellings.[1]

The slate appears to lie in a vein. The

[1] 'Bull's-eyes,' due to a nucleus of iron pyrites, according to Survey memoir to accompany Sheets 200, 203, 204, 205, and part of Sheet 199, p. 14.

cleavage is within 2° of the vertical, inclining northerly; it is very micaceous and splits very thin. The black colour may in part be due to the carbon present.

Specimens 1, 2, and 6 are from this quarry.

It is a striking fact that the cleavage crosses the bedding at a high angle, as I believe it does in most of the best roofing slates.

The Madramma slate is of a similar nature. The quarry is near Leap; it was not visited, the specimen being kindly obtained for us by Mr. Thomas Connell, the proprietor of the Eldon Hotel, Skibbereen. The cleavage crosses the bedding in this specimen also.

That cleavage is always associated with intense pressure, and this, again, with great thickness of deposit, is strongly in evidence in these interesting sections in County Cork. In England, slates younger than the Silurian are only found in Devonshire and Cornwall, on the line of this same truncated and buried mountain range, which, commencing in the South of Ireland, passes through Devonshire, crosses the English Channel, and culminates in the plateau of the Ardennes.

The enormous thickness of the Carboniferous and Old Red Sandstone beds in the South of Ireland is well set forth in the various Memoirs of the Geological Survey of Ireland.[1] Jukes originally thought that the Carboniferous Limestone

[1] See section on p. 82: Explanation to accompany Sheets 187, 195, and 196 of the Map of the Geological Survey of Ireland.

extended over the whole of the South of Ireland, but finally came to the conclusion 'that the Carboniferous Shale must be taken to be contemporaneous with the Carboniferous Limestone.'[1]

Jukes says: 'It can be shown that all this vast series of beds was deposited on the slowly subsiding and rather irregular surface of a previously existing land, made of the Lower Palæozoic rocks, and that the depression commenced first on the south or south-west, and continued there for a long time during the deposition of the great mass of the Old Red Sandstone, before it began to affect the centre of Ireland, where the Old Red Sandstone is comparatively thin or does not exist at all.'[2]

Description of the Rock Specimens from County Cork

Slates and Slate Rock.—The accompanying Table (No. 1) gives our analyses of the typical specimens taken from the slates and associated beds of County Cork. They are numbered 1, 2, 3, 4, 5, 6, 9, 10, 11. Specimens Nos. 7 and 8 were omitted as unnecessary. Nos. 1, 2, 6 are Benduff roofing slate from different parts of the same quarry. No. 6 (Plate XXXI.), being the first quality of slate, was selected for microscopical examination. Mr. Hutchings reports as follows:

[1] *Loc. cit.* p. 35.

[2] 'On the Mode of Formation of some of the River Valleys in the South of Ireland' (*Q. J. G. S.*, 1862, p. 384).

TABLE 1.—TABULAR STATEMENT OF ANALYSES OF SLATES AND ROCKS FROM COUNTY CORK

Material for Analysis was dried at 100° C.

—	No. 1	No. 2	No. 3	No. 4	No. 5	No. 6	No. 9	No. 10	No. 11	—
	Benduff slate, County Cork, N. side of vein. Cleavage crosses the bedding; a roofing-slate	Benduff slate of fine quality from another part of the quarry. The cleavage crosses the bedding	Slate rock from Quarry No. 2, E. side of Clonakilty Lough, County Cork. *Not* a roofing slate. The bedding and cleavage coincident	A gritty fissile rock, Quarry No. 1, E. side of Clonakilty Lough. Fissility and bedding in the same plane	Madramma slate, Leap, County Cork. A roofing slate. The cleavage crosses bedding	Benduff slate. A fine quality of roofing slate from another part of quarry	A greenish-grey soft rock, of fissile character, from a quarry on W. side of Clonakilty Lough	Gritty bed. The bedding and cleavage coincident. From same locality as No. 9	From the same quarry as No. 10. The bedding and cleavage coincident. It outwardly resembles No. 9	
Total SiO_2	58·47	56·76	61·53	69·03	55·83	53·60	66·32	80·25	69·36	Silica
TiO_2	0·92	0·91	0·75	0·59	0·91	0·88	1·26	0·75	1·05	Titanic oxide
Al_2O_3	20·13	19·77	21·26	19·89	24·31	21·89	18·46	10·19	16·07	Alumina
Fe_2O_3	—	0·71	2·73	0·07	0·33	0·07	1·05	0·45	0·73	Ferric oxide
FeS_2	0·54	0·95	—	—	0·63	0·97	—	—	—	Pyrites
FeO	6·02	5·26	1·84	2·68	4·39	4·62	3·43	2·49	3·88	Ferrous oxide
MnO	0·38	0·24	0·27	—	0·34	0·22	—	0·09	0·11	Manganous oxide
CaO	1·29	2·40	0·20	0·05	0·70	3·30	0·14	0·05	0·12	Lime
BaO	0·05	0·08	0·06	0·06	0·05	0·06	0·04	0·05	0·03	Baryta
MgO	1·56	1·76	2·87	0·48	1·21	1·56	1·46	0·85	1·45	Magnesia
K_2O	3·82	3·87	3·67	3·76	4·77	4·33	3·66	1·66	3·26	Potash
Na_2O	0·79	0·85	1·12	1·20	0·78	0·75	0·59	1·03	0·68	Soda
CO_2	1·31	2·18	—	—	0·97	3·16	—	—	—	Carbonic acid
P_2O_5	0·07	0·06	0·10	0·02	0·06	0·06	0·01	—	—	Phosphoric acid
C	0·84	0·74	traces	—	0·94	0·75	traces	—	—	Carbon
Combined Water	4·01	3·87	3·60	2·32	3·58	3·93	3·43	2·13	3·14	
	100·20	100·41	100·00	100·15	99·80	100·15	99·85	99·99	99·88	
Sp. Gr.	2·805	2·782	2·787	2·840	2·777	2·790	2·662	2·650	2·696	

The CO_2 is somewhat in excess of the amount required for the lime as calcite, so that probably some ferrous or calcic magnesian carbonate is present in Nos. 1, 2, 5, and 6.

'No. 6, a very fine-grained clay-slate, dark with organic pigment and iron ores. It is just a minutely woven mass of micaceous and chloritic material with small grains of quartz. Larger grains of quartz here and there are drawn out in the direction of cleavage by great pressure, and there are similarly elongated lenticles of chlorite and a mixture of chlorite and calcite.

'Not much clastic mica can be made out, owing to the fine grain and to its concealment in the felted mass of the newly formed materials.

'The usual minute "clay-slate needles" of rutile are extremely abundant throughout the rock. Small grains and crystals of calcite are pretty evenly diffused, as are also the grains of iron ore. [These grains are pyrites.]

'The sections examined show a rock derived from very fine-grained sediment, which has undergone great pressure as well as great chemical alteration, but has not been affected by any sort of contact action.'

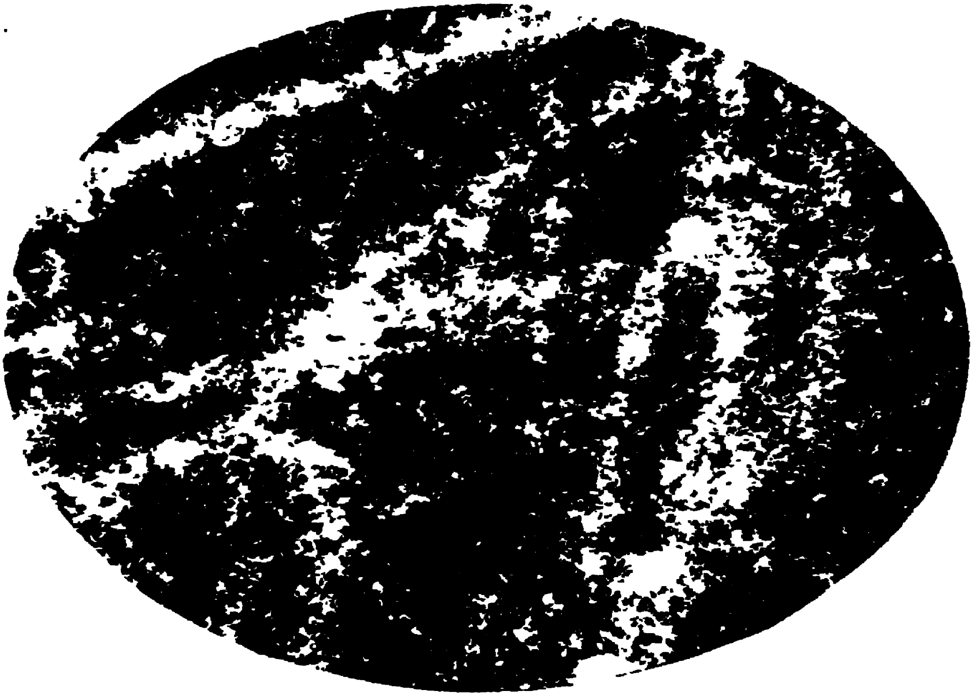
No. 5 is a Madramma roofing slate. This Mr. Hutchings describes as 'a fine-grained, highly laminated slate, with a good deal of dark organic pigment, and much opaque iron ore in grains and crystals.

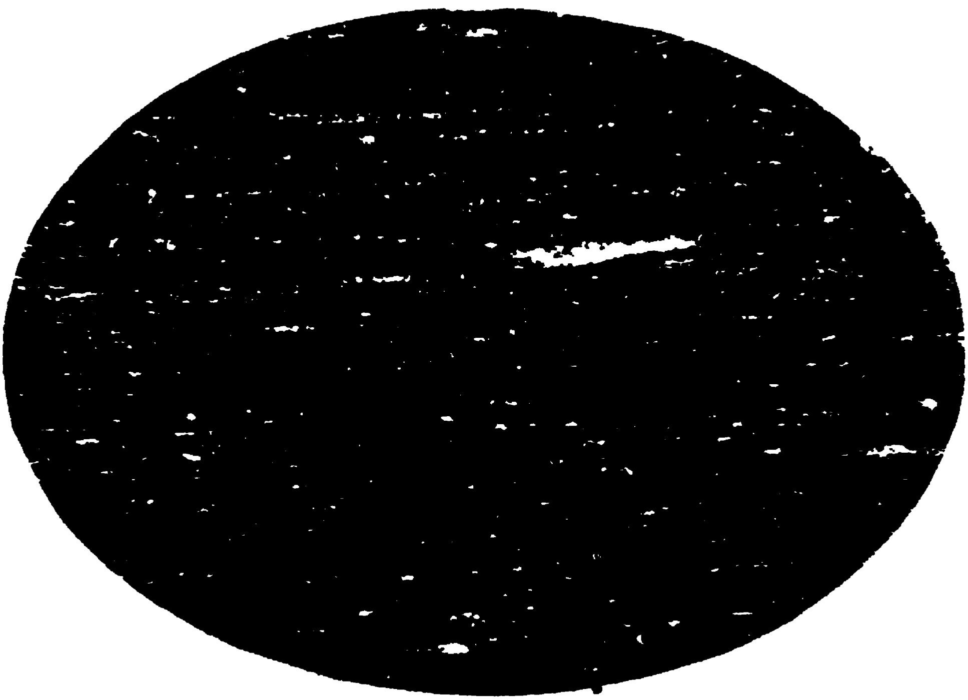
'The main mass is mica, with a large amount of chloritic mineral.

'There is a good deal of quartz, partly in good distinct grains, but also to a large extent so finely

PLATE XXXI.

A





disseminated that its quantity cannot well be judged without a chemical determination.

'Original clastic mica is still distinguishable, and probably much more is hidden away in the dense mass of newly formed micaceous material and chlorite.

'There is a great abundance of rutile and minute "clay-slate needles."

'The mica and the larger bits of quartz again show evidence of the great pressure and drawing out which the rock has suffered.'

A reference to the Table of Analyses will show the presence of a considerable, though variable, amount of carbonic acid in the Benduff slates, and a lesser in the Madramma, though in the latter case only one specimen was analysed. Doubtless the carbon is from the organic matter originally present.[1]

Comparing these analyses with those of the Phyllades of the Ardennes in Table II., there exists a striking similarity in the proportions of silica and alumina present; but carbon is absent in the Ardennes rocks that we analysed, and these, again, showed greater mineralogical change.

The next specimen of slate rock from County Cork is No. 3 of Table 1. It is a true slate, but not fit for roofing purposes.

This is the mineralogical description: 'A fine-grained slaty rock, composed of white mica, quartz,

[1] It is recorded by Hillebrand in dark slates from the United States.

and pale chloritic minerals, with many grains of opaque iron ore. There are also some small crystals of rutile (not very many) and a few flakes of micaceous ilmenite. It is probable that the opaque iron ore is also ilmenite to a considerable extent.

'The mica is very nearly all flat in one plane, and is all newly formed, as far as can be made out. If any original clastic mica still remains, it is wholly shrouded in the mass of new material.

'The quartz is mostly in elongated grains, and the lenticles are drawn out in the same plane in which the mica lies flat. There is evidence that the rock has undergone very great pressure and rolling out.'

The chemical analysis of this rock shows that, excepting for the larger proportion of silica present and the less FeO, together with the entire absence of CO_2, it is similar in character to the preceding roofing slates. Whence, then, arises the difference in quality? Is it that the bedding and cleavage are coincident?

We will return to this query further on.

Sp. Gr. 2·787. Porosity, 3·2 per cent.

Associated Grits and Shales.—We now come to a class of rock making up the bulk of the Old Red Sandstone and Carboniferous Slate series, with which the true slates are associated.

There are many veins of slate occurring in the Old Red, mentioned in the Survey memoirs, of purple, red, and green shades of colour. These quarries do not appear to have been much worked,

probably from the difficulty of getting roofing slates from them of satisfactory sizes without great waste.[1]

Specimens Nos. 4, 9, 10, 11 represent a large proportion of the bulk of the deposits; but the beds are of very variable character, and we have for the sake of brevity confined ourselves to those specimens only that bear directly upon our subject of slaty-cleavage.

No. 4 is a gritty fissile rock from Quarry No. 1, east side of Clonakilty Lough, in the Lower Old Red Sandstone series. Fissility and bedding are in the same plane. Microscopically described, it is 'a rather coarse-grained felspathic and very micaceous grit.

'The quartz and felspars are in good-sized angular clastic grains, compacted together in the usual manner, *and more or less surrounded and cemented by a fine-grained material of felted flakelets of newly formed mica, chlorite, &c.*

'The felspar is rather abundant—almost all of it well-twinned plagioclases, apparently largely oligoclase.

'The rock is banded by layers in which mica

[1] Several quarries are mentioned in the memoir already referred to on p. 17. Small abandoned quarries are noted in Shirkin Island. As Jukes observes ('Explanation to accompany Sheet 192, and part of 199,' p. 41), 'the characters which distinguish an ordinary clay-slate, such as a geologist would call a "good slate," from that which makes a first-class roofing slate, although of high importance economically, are, nevertheless, so minute and imperceptible to the eye as scarcely to be appreciated until a quarry is actually opened and the matter brought to a practical test.'

largely predominates, alternating with others which are relatively free from it. Its fissility is due to this and to the fact that the mica flakes are largely arranged in one plane.

'A large proportion of the mica is in the original clastic flakes of good size; most of this is muscovite, but it is still possible to identify a considerable amount of more or less altered biotite, both optically, and by the alteration-products in the flakes.

'The finer portion of the original sediment is now completely regenerated, as usual, and forms the felted micaceous material in which the coarser mica and the quartz and felspar are bedded. From it all biotite has disappeared.

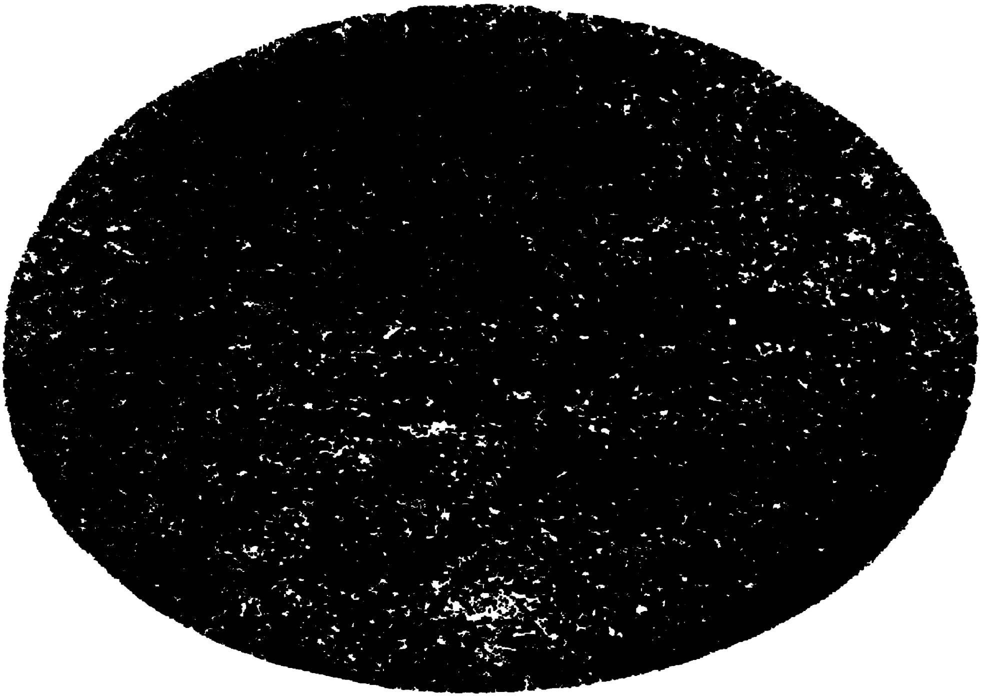
'There are the usual zircons and tourmalines, and a large amount of rutile crystals derived from the alteration of the biotite.'

No. 9 (Plate XXXII.) is from a quarry on the west side of Clonakilty Lough, near the junction of the Upper and Lower Old Red Sandstone. It was described by one of us in the field as 'a greenish grey soft rock of fissile character.' This seems to have a cleavage impressed upon it at a very small angle to the bedding, which gives the split surface a minute ripple-marked appearance. There is so much fine material in these sandstones that they may be looked upon as approaching true shales.

An examination of any of the best buildings in this part of the South of Ireland emphasises this character of the rocks. The freestones locally used for quoins and dressings are of a soft nature,

PLATE XXXII.

C



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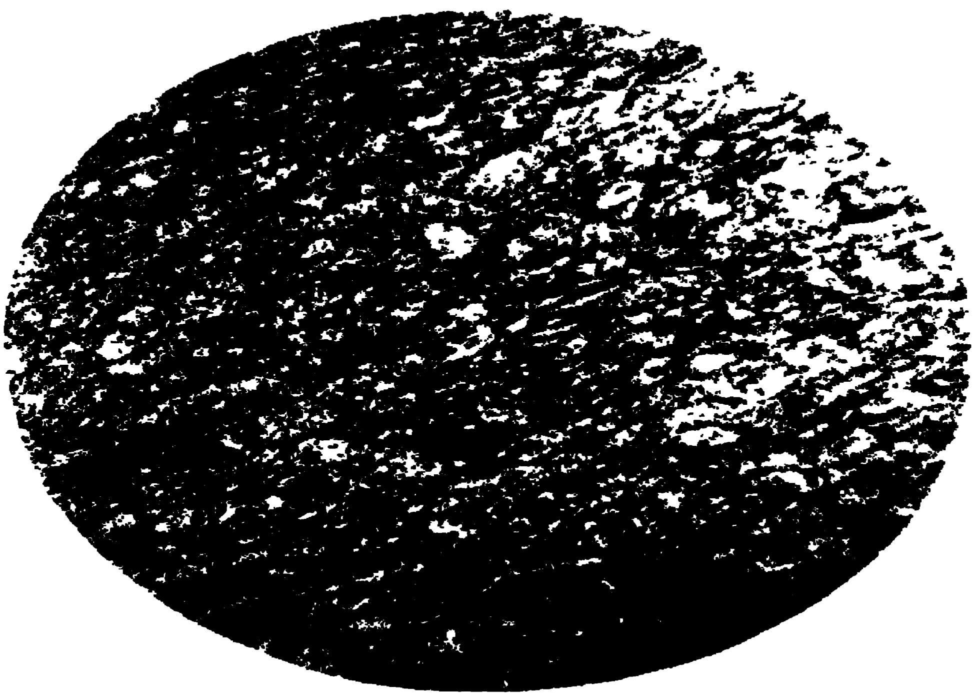
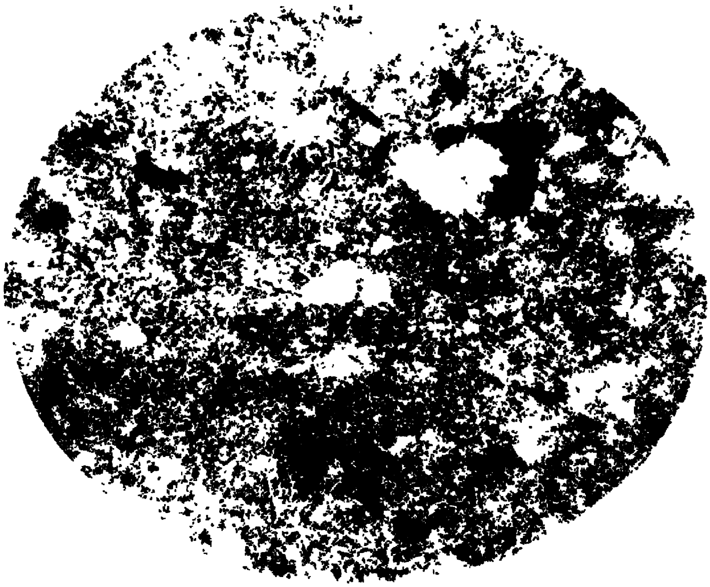


PLATE XXXIII.

E



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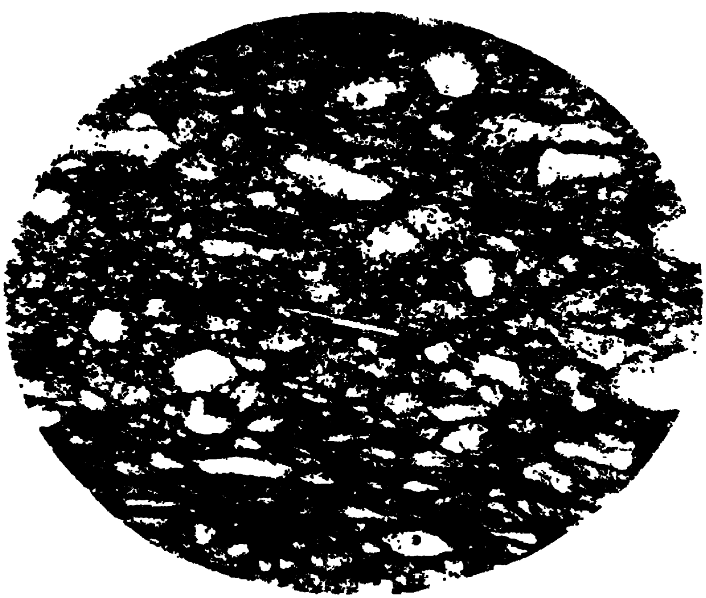

easily disintegrated by the weather; hence for this purpose in the best buildings limestone from the Carboniferous system is imported. In the very splendid new Catholic church in Clonakilty the dressings and tracery are of Irish grey granite, the walling of a fine local sandstone or mudstone, and the spire of limestone.

Only the very best beds can be used for building purposes. The rough fence-walls, built of the local rock, are usually covered with a cement coping, which assists very much to preserve the wall from decay.

The microscopical description of No. 9 is as follows:—

'A fine-grained very micaceous grit. It is again banded by very micaceous layers, alternating with quartzy ones.

'The quartz here is very little mixed with felspar; much of it is of a very small size, and some of the grains are more or less corroded and blended away into one another.

'The mica is partly clastic and partly newly formed. The latter much predominates, and is felted with a large proportion of impure chloritic material.

'There is little biotite; the mica is partly muscovite and partly the more indefinite, impure mica, developing towards muscovite.

'There is a large amount of rutile in the micaceous layers.'

Specimen No. 10 is from the same quarry. It

is from a gritty bed, the bedding and cleavage being coincident:—

'A micaceous, very much compacted grit or quartz. There is a good deal of calcedonic silica among the quartz; a fair amount of felspar.

'The mica is all muscovite; a lot of distinct good-sized flakes of original clastic material, and a more fine-grained portion, consisting mainly of newly formed materials, but in which more or less clastic material is included.

'There are also some chlorite, zircons, and rutile grains.

'In this particular section the proportion of mica to quartz and felspar is not large, and there is no great effect of lamination or fissility; but the flakes, both old and new, are almost all flat in the one plane, and there is evidence both in the mica and the quartz grains that this rock also has undergone much pressure.'

Specimen No. 11, Plate XXXIII., is from another bed in the same quarry as Nos. 9 and 10. Outwardly it resembles No. 9. The bedding and cleavage are coincident.

'Again a micaceous grit or quartzite. It is finer in grain than No. 10, and less felspathic, and the amount of chalcedonic (secondary) silica in among the quartz grains is larger.

'The proportion of mica is higher than in No. 10, and as it is not evenly diffused, but lies mainly in separate bands, the effect of lamination is more pronounced.

'There are grains of zircon and rutile, and a good many fragments of tourmaline.'

Differences between the Slates and Grits.—An examination of the table of analyses and comparison of the chemical composition of the slates and sandstones appear to point to the same sedimentary origin for both. Not only so, but the mineral constituents in them are remarkably similar. Silica predominates in the grits. This silica is present more in the form of pronounced quartz grains and fragments. The mineral changes the two sorts of rocks have undergone are also similar in kind, if not in degree. The original muddy matrix in which the coarser grains of quartz were embedded appears to have become less perfectly mineralised, and a large amount of impure chloritic material and impure mica is often present, as in No. 9.

Description of Plates illustrating Slates and Rocks of County Cork

Plate XXXI.—Finest quality of Benduff roofing slate (No. 6 of Table). Fig. A, cut parallel to cleavage plane; × 88½. Fig. B, cut transverse to cleavage plane; × 88½.

Plate XXXII.—A fine fissile slaty grit from quarry, west side of Clonakilty Lough (No. 9 of Table). Fig. C, cut parallel to fissile plane; × 88½. Fig. D, cut transverse to fissile plane; × 88½.

Plate XXXIII.—From the same quarry as the preceding (Plate II. No. 11 of Table); a similar rock to No. 9, but coarser in grain. Fig. E, cut

parallel to fissile plane; $\times 88\frac{1}{2}$. Fig. F, cut transverse to fissile plane; $\times 88\frac{1}{2}$.

In the following tables we have compared a slate with a fissile sandstone, giving their probable mineral composition as regards the chief minerals.

No. 5. Madramma Slate

Mica $(KNa)_2O$	50·88
Chlorite $(FeMnMg)O$	11·17
Rutile	0·91
Pyrites	0·63
Calcite	1·12
Ferrous Carbonate	1·26
Quartz	30·01
Rest	4·37
	99·80

Porosity = 2·2 per cent.

The full analysis of No. 5 left a balance of CO_2 after calculating the calcite, which we venture to assign to ferrous carbonate.

In a Belgian phyllade M. Renard gives 47·24 and 12·22 for the respective percentages of mica and chlorite, along with 34·61 of quartz (see transcript of M. Renard's analysis in 'Phyllades of the Ardennes compared with Slates of North Wales:' 'Proc. Liverpool Geo. Soc., vol. viii., 1898).

No. 10. Gritty Band from Quarry, West Side of Clonakilty Lough

—	SiO_2	TiO_2	Al_2O_3	Fe_2O_3	FeO	MnO	MgO	K_2O	Na_2O	H_2O	Sum
Mica . .	6·23	—	5·30	—	—	—	—	0·84	0·54	0·62	13·53
Felspar .	6·08	—	1·72	—	—	—	—	0·82	0·49	—	9·11
Chlorite .	2·05	—	1·16	—	2·49	0·09	0·85	—	—	0·82	7·46
Quartz .	63·53	—	—	—	—	—	—	—	—		63·53
Clay . .	2·36	—	2·01	0·45	—	—		—		0·70	5·52
Rutile .	—	0·75	—	—				—	—	—	0·75
Rest . .	—	—	—	—		—	—	—	—	—	0·09
Sum .	80·25	0·75	10·19	0·45	2·49	0·09	0·85	1·66	1·03	2·14	99·99

Porosity = 5·6 per cent.

POROSITIES

To help to elucidate the structural peculiarities of slates and sandstones and the differences between them which we have seen exist in rocks that have much the same mineralogical composition, and have apparently been exposed to similar conditions of pressure and temperature, we have determined the porosities of some typical specimens.

The method adopted is that given by Mr. C. Moore, F.I.C., in his very suggestive paper read to the Geological Society of Liverpool[1] on February 8, 1898, entitled 'The Chemical Examination of Sandstones from Prenton Hill and Bidston Hill.' Our plan differed from Moore's in this respect, that the determinations were made on air-dried rock, and not on rock which had been first dried at 100° C. *before weighing* and immersion in water. The effect of drying before immersion is, of course, to raise the porosity figure, since this expels adventitious moisture. In the case of No. 9 sandstone drying the rock raised the porosity from 7·2 to 8·6 per cent.; but with Velenhelli slate the porosity remained the same. The closer texture of slate, rendering it less accessible to adventitious moisture, will explain this.[2] The mean weight for

[1] *Proceedings of the Liverpool Geo. Soc.*, 1897 98, p. 248. See also 'The Study of the Volume Composition of Rocks and its Importance to the Geologist,' by Mr. Moore, being the Presidential Address to the Liverpool Geo. Soc., 1901 1902. Also Part II. 'The Examination of an Igneous Intrusion' (*Pres. Ad.* 1902–1903).

[2] There have been various experiments made from time to time by engineers and architects, and others interested in building-stones

one of the eighteen pieces of rock used for the determinations given in these pages was 87 grammes.

All the specimens, it may be stated, had lain in the warm air of a room for some months, and were considered sufficiently air-dried for comparative tests such as these are intended to be. The duration of immersion in boiled distilled water was in all cases twenty-four hours.

The figures are percentage by *volume*.

Table of Porosities

		Per cent.
No. 1.	Benduff. County Cork. Irish roofing slate	1·5
No. 2.	Do. do.	1·3
No. 3.	Slate rock, east side of Clonakilty Lough, County Cork. (Not a roofing slate)	3·2
No. 4.	Gritty fissile rock (Quarry No. 1), east side of Clonakilty Lough, County Cork. Fissility and bedding on the same plane	2·6
No. 5.	Madramma slate, County Cork. A roofing slate. Cleavage crosses bedding	2·2
No. 9.	A greenish light-grey soft rock, of fissile character, west side of Clonakilty Lough, County Cork	7·2
No. 10.	Same locality County Cork. Bedding and cleavage coincident	5·6
No. 11.	Same locality as No. 10. (It outwardly resembles No. 9)	7·8
	Welsh slate. Moel Tryfaen, Alexandra Quarry	0·5
	Welsh slate (banded), Llansantffraid Glyn Ceiriog, Llangollen	0·8
	Welsh slate, Velenhelli	0·3
	Welsh green foliated schist, Beaumaris Road, Anglesey. Sp. gr. 2·768	0·77

to test their absorption qualities. These results are generally given in weight of water absorbed. See the *Builder*, June 30, 1894, p. 503, and *Proc. Inst. of C. E.*, 'Beare on Building-stones,' vol. cvii. pp. 361 369. For our purposes we adhere to the volume determinations, as being simpler and giving exactly and directly the proportions between the interstitial space and the space occupied by the solid matter of the rock.

	Per cent.
Morte slate, Mort Point, North Devon. Sp. gr. 2·821	1·6
Slaty rock, Parade, Lynmouth. Sp. gr. 2·755 . .	0·7
Combemartin Bay. Silicified band. Sp. gr. 2·806 .	0·5
Grit, Hunter's Inn. Sp. gr. 2·652	2·9
Band of very soft Triassic sandstone occurring in a bed of red marl at Storeton, near Liverpool. Sp. gr. 2·408	14·8
Keuper sandstone from Caldy Grange Quarry. Sp. gr. 2·606	17·4
Lower Bunter sandstone from Bore No. 2, Hoylake and West Kirby Waterworks, 923 feet below surface. Sp. gr. 2·615	13·9
Millstone grit from quarry at Kerridge, Rainow, near Macclesfield. Sp. gr. 2·59	2·8

An examination of these results shows a marked inverse relation between the porosities and the strengths of the rocks tabulated. The Welsh slates are the most compact, and are as a rule the strongest. The percentage of porosities ranges from 0·3 in Velenhelli [1] to 0·8 in Llangollen slates. The Irish roofing slates vary from 1·3 to 2·2. The grits from County Cork, of much the same mineral composition and affected by great lateral pressure, being turned up nearly vertical, range from 2·6 to 7·8, the lower percentage being the stronger rock, the higher, though extremely fissile, being soft and having little transverse strength.

Again, the form and size of the component grains exercise a great influence upon the ultimate strength of the rock. Both sandstones and slates are built-up structures, and it is easy to see that a sandstone composed of subspherical grains can

[1] This was a fragment of the slate which is mentioned on p. 225 as having the transverse strength of English oak.

never attain the strength of one made up of grains with one axis longer than the other. Not only are the areas of contact greater in the case of an aggregation of long grains, but these lend themselves to more perfect bonding by overlapping. The mode in which these particles are laid down sub-aqueously or by the wind produces a bonded structure, the grains being fitted together like bricks. A deposit of mineral matter, such as secondary quartz, forms the mortar cementing them together, and, other things being equal, the strength of the resulting rock is in proportion to the quantity present.

It was these considerations that induced us to pay some attention to the porosities of slates as compared with other classes of rock. The result shows that the interstitial space in slates, especially the more compact varieties, is small as compared with sandstones. The quality of comparative non-absorbency of slates is taken advantage of by builders, who sometimes build in a layer of slates to form a 'damp-proof' course to prevent walls becoming damp by the capillary attraction of ordinary brick or stone, which sucks up moisture from the soil. This defect is known to most people who live in old houses, erected when our ancestors did not indulge in such luxuries as 'damp-proof' courses, which are now, I may say, more generally made of asphalte.

To resume our consideration of relative strengths. We have seen that slates are built up of microscopic

overlapping scales, in which the contacts are more continuous and perfect than in granular rocks. I believe that in the case of the hardest Welsh slates the cement is of a more silicious character than usual, and that the interstitial spaces are extremely minute.[1]

From these experimental investigations it appears pretty well demonstrated that pressure alone is incompetent to produce slaty-cleavage. It is impossible to dissociate slaty-cleavage from the contemporaneous deposition of secondary minerals during shearing. The relative strength and perfection of the cleavage depend upon three factors, namely:—

1st. The fineness, homogeneity, and mineral composition of the original mud.

2nd. The uniformity and intensity of the shearing forces.

3rd. The completeness with which the secondary minerals are developed, compacted and felted together.

North Devon Rocks.—Among the specimens tabulated for porosity are some from the neighbourhood of Ilfracombe. In that interesting neighbourhood other phases of slaty-cleavage may be studied. One of the most striking is the effect of lateral

[1] As showing the extremely absorbent nature of some otherwise excellent sandstones used for building, see 'Experiments on the Circulation of Water in Sandstone' (*Proc. of Liverpool Geo. Soc.*, vol. iv. Part VI. pp. 434–47). A solid siphon of Triassic sandstone was exhibited by Mr. Reade at the reading of the paper, and a vessel filled with water drained of its contents simply through the capillary tubes or interstices of the sandstone siphon.

pressure upon the bands of grit that traverse the impure slaty rocks. Though the pressure has produced a cleavage in the rocks that affects the structure of immense masses and gives a striking individuality to the scenery, the fissility of the slaty beds is not shared by the grit bands. These are in the first stage affected by imperfect cleavage, the planes being far apart. Where the bands are thinner they are frequently broken up into fragments. In the final stage these fragments are turned on end at a high angle, and lie parallel to each other, resembling a bed or row of nearly vertical nodules. An examination proves, however, that they are not nodules, their clastic structure being well preserved. Round these fragments the foliated slaty rock has flowed in the same manner as the mica and chlorite have flowed on a minute scale, round the larger grains of quartz, as seen in microscopic sections of slates.

Reprints of former Tables of Chemical Analyses and Reproduction of accompanying Illustrations

For the purposes of comparison I have reproduced the Tables of Chemical Analyses of our three published papers already several times referred to in this chapter; accompanying them are also the micro-photographs with which they were illustrated.

It would have taken up too much space to reproduce the descriptive details of mineralogical

characteristics of each specimen; those who desire it can consult the originals, viz.:

'The Phyllades of the Ardennes compared with the Slates of North Wales,' Part I. (*Proceedings Liverpool Geo. Soc.*, 1897–98).

'The Phyllades of the Ardennes compared with the Slates of North Wales,' Part II. (*ibid.*, 1899–1900).

'The Green Slates of the Lake District' (*ibid.*, 1900–01).

'*Phyllades of the Ardennes compared with the Slates of North Wales*' (*Table* 2)

The following is a list of the specimens of rocks and their localities to which the accompanying table refers:—

No. 1. Blue quartzite, Specimen A, collected near Fépin, Revinien, Valley of the Meuse.

No. 2. Quartzite, Specimen B, collected near Fépin. Devonian inférieure. Valley of the Meuse.

No. 3. Phyllade Ottrélitifère. Revinien, Monthermé, Valley of the Meuse.

No. 4. Phyllade Aimantifère (green), from below Monthermé, Valley of the Meuse.

No. 5. Slate quarry between Haybes and Fépin. Phyllade Devillien (violet). Valley of the Meuse.

No. 6. Penrhyn slate (1). Cambrian.

No. 7. Penrhyn slate (4). Do.

No. 8. Egryn slate, Barmouth. Cambrian.

No. 9. Slate from quarry above Llanllyfni, near Pen-y-groes. Silurian.

No. 10. Slate from Velenhelli, near Llanberis. Cambrian.

No. 11. Delabole slate. Cornwall.

No. 12. Mathews red slate. Vermont, U.S.A.

TABLE 2.—'THE PHYLLADES OF THE ARDENNES COMPARED WITH THE SLATES OF NORTH WALES,' PART I

—	No. 1 Quartzite A	No. 2 Quartzite B	No. 3 Phyllade, Ottrélitifère, Blue Black	No. 4 Phyllade Aimantifère, Green. Analysis in duplicate for seven constituents		No. 5 Phyllade Devillien, Violet	No. 6 Penrhyn Slate (1), Cambrian, Purple	No. 7 Penrhyn Slate (4), Cambrian, Purple	No. 8 Egryn Slate, Cambrian, Barmouth, Green	No. 9 Llanfyllni Slate, Silurian, Green	No. 10 Velenheli Slate, Cambrian, Llanberis, Blue	No. 11 Delabole Slate, Cornwall, Grey	No. 12 Vermont Slate, U.S.A., Red	—
				No. 1	No. 2									
Total SiO_2	92·18	87·30	57·80	57·89	57·73	60·56	57·75	63·01	59·16	57·27	63·30	56·75	60·30	Silica
Al_2O_3	3·11	3·61	23·96	20·40	20·47	19·72	16·44	16·28	18·42	17·60	14·99	21·81	13·75	Alumina
Fe_2O_3	0·45	1·40	3·10	4·93	5·28	6·57	10·84	8·42	7·67	4·70	8·75	1·36	5·03	Ferric oxide
FeO	1·47	3·22	3·40	3·98	4·10	1·86	1·66	0·66	2·92	6·40	1·53	7·20	0·86	Ferrous oxide
MnO	trace	0·27	0·42	trace	—	0·11	0·25	0·42	0·14	0·46	0·25	trace	trace	Manganous oxide
TiO_2	0·41	0·37	1·01	0·89	0·81	0·55	1·27	0·58	0·17	1·20	1·04	0·71	trace	Titanic oxide
CaO	—	—	0·43	—	—	trace	0·23	0·72	0·04	0·17	1·08	0·84	3·62	Lime
MgO	0·43	1·10	1·59	2·42	2·42	1·87	2·36	1·95	2·54	2·28	1·63	2·48	5·20	Magnesia
K_2O	*0·84	0·22	3·02	4·74	4·79	4·64	4·23	2·98	4·01	3·79	2·84	3·72	3·84	Potash
Na_2O	—	0·59	0·85	1·26	1·13	0·55	1·30	2·14	0·80	1·20	1·92	0·97	0·72	Soda
CO_2	—	—	—	—	—	—	—	—	—	—	—	—	3·77	Carbonic acid
P_2O_5	—	—	—	trace	—	0·09	0·21	trace	0·07	0·11	0·07	trace	trace	Phosphoric oxide
Carbon, approximately	0·25	—	0·35	—	—	—	—	—	—	—	—	—	—	
Combined Water	1·13	1·85	4·22	3·08	3·08	3·85	3·35	2·92	4·00	4·47	2·88	4·23	2·91	
	100·25	99·93	100·15	99·59	99·81	100·37	99·89	100·08	99·94	99·65	100·18	100·07	100·00	
Silica as Quartz per cent.	—	—	32·93	33·16		35·36	34·66	42·26	36·45	33·71	43·03	33·24	38·76	
Sp. Gr.	2·695	2·690	2·878	2·854		2·816	2·852	2·852	2·840	2·838	2·883	2·841	2·828	

The occurrence of phosphoric acid in some of these slates is noteworthy. J. H. Coste (Berichte, 1896) records 0·11 per cent. in the slate from Dinorwic, near Llanberis.

* Chiefly K_2O.

OF NORTH WALES,' PART II.

	No. 1	No. 2	No. 3	No. 4	No. 5	No. 6	No. 7	No. 8	No. 9	No. 10	No. 11	No. 12*	
	Best slate, Alexandra Quarry, Moel Tryfaen	Banded slate, Llansantfraid, Glyn Ceiriog, near Llangollen	Moel Ferna slate	Green slate from quarry between Camlyn and Cemmaes, Anglesey	Slate rock (crinkled), Llanrhwydroes, Anglesey	Slate in dyke, east of Camlyn Bay, Anglesey	Slate penetrated by sheets and veins, Yr-henborth, Anglesey	Green slate, Buttermere, English Lake district	Slate from anticlinal at Aberdovey, North Wales	Schist from Holy Island, Anglesey	Schist from South Stack, near Lighthouse, Holyhead, Anglesey	Greenish-grey infiltrated rock penetrating as a sheet No. 7 slate, Yr-henborth, Anglesey	
Total SiO_2	60·17	56·71	56·92	57·23	61·65	62·76	55·34	27·88	53·23	79·80	76·77	80·59	Silica
Al_2O_3	18·89	14·43	16·41	20·43	16·82	15·06	20·87	9·93	23·50	9·40	11·15	6·45	Alumina
Fe_2O_3	6·17	1·98	0·53	1·33	6·61	4·60	7·07	0·63	0·79	1·29	1·71	4·77	Ferric oxide
FeO	0·95	3·65	3·52	5·64	1·39	1·79	1·19	5·28	7·89	2·26	2·32	0·73	Ferrous oxide
FeS_2	—	2·64	3·97	—	—	—	—	—	—	—	—	—	Iron pyrites
MnO	0·20	0·06	0·16	0·05	1·07	0·67	0·36	0·48	0·20	0·22	0·12	0·81	Manganous oxide
TiO_2	1·15	0·70	0·90	0·89	0·70	0·66	1·94	0·77	0·96	1·12	0·74	0·34	Titanic oxide
CaO	1·75	3·83	2·94	1·54	0·15	0·96	0·78	23·69	none	0·11	0·09	3·64	Lime
BaO	0·04	0·04	0·03	0·02	0·03	0·06	0·07	0·02	0·04	0·02	0·03	none	Baryta
MgO	1·85	3·47	3·14	2·09	2·02	6·14	1·58	6·28	2·38	1·05	1·06	0·82	Magnesia
K_2O	2·76	2·61	3·27	2·39	2·90	0·08	4·76	0·97	3·83	1·51	2·85	0·24	Potash
Na_2O	1·39	2·59	1·47	3·97	3·36	3·56	2·80	0·75	1·20	1·60	1·05	0·73	Soda
CO_2	1·04	3·71	2·68	—	—	—	—	18·26	—	—	—	—	Carbonic acid
SO_3	none	0·12	0·10	0·03	none	0·04	trace	0·12	0·03	none	none	—	Sulphuric acid
P_2O_5	0·11	0·05	0·09	0·54	0·08	0·12	0·13	0·13	none	0·07	0·11	—	Phosphoric acid
Carbonaceous matter, approximately	—	0·77	1·54	traces	—	—	—	—	traces	—	traces	—	
Combined Water	3·70	2·74	2·21	3·94	3·15	3·49	3·39	4·59	5·82	1·79	2·41	1·06	
	100·17	100·10	99·88	100·09	99·93	99·99	100·28	99·78	99·87	100·24	100·41	100·18	
Uncombined SiO_2, chiefly Quartz	—	—	—	33·49	—	—	—	—	—	—	—	—	

* Compare 'Notes on Excursion to Anglesey,' T. M. Reade; also 'Examination of a few Anglesey Rocks,' Holland and Dickson (*Proceedings Liverpool Geological Society*, Part II. vol. vi.).

TABLE 4.—LAKE DISTRICT GREEN SLATE.—TABLE OF ANALYSES.

Material for Analysis was dried at 100° C.

—	No. 1 Elterwater Green Slate Co., Limited, Westmoreland	No. 2 Buttermere Green Slate Co.'s Quarry at Honister Pass, Buttermere	No. 3 Buttermere green slate, No. 1 best ton slate, same quarry as No. 2	No. 4 Buttermere green slate, No. 5 best ton slate, deep olive green, same quarry as No. 2	No. 5 Tilberthwaite Quarry, Coniston, best dark green, worked by J. Stephenson & Co., of Kendal	No. 6 A Welsh slate, Velenhelli, second green, worked by Assheton Smith, Esq.	—
Total SiO_2	52·67	52·34	53·50	54·02	50·16	63·06	Silica
TiO_2	1·18	1·05	0·97	1·16	1·03	0·73	Titanic oxide
Al_2O_3	12·63	11·94	12·75	14·66	17·85	18·03	Alumina
Fe_2O_3	1·58	2·79	2·74	1·28	1·65	2·24	Ferric oxide
FeO	6·95	7·06	5·97	5·87	6·36	4·07	Ferrous oxide
MnO	0·19	0·39	0·35	0·38	0·48	0·30	Manganous oxide
CaO	5·78	6·36	6·46	5·52	3·67	0·81	Lime
BaO	0·02	0·02	0·03	0·03	0·03		Baryta
MgO	6·37	6·10	5·59	4·48	6·35	2·21	Magnesia
K_2O	2·47	1·03	1·13	1·82	2·06	3·07	Potash
Na_2O	1·31	1·78	1·82	2·18	3·11	1·51	Soda
†CO_2	5·41	4·51	4·48	3·26	2·45	none	Carbonic acid
SO_3	none	0·14	0·07	0·10	0·10	0·09	Sulphuric acid
P_2O_5	0·16	0·12	0·13	0·20	0·16	0·06	Phosphoric acid
Combined Water	3·54	4·70	4·13	3·93	4·67	3·62	
	100·26	100·33	100·12	99·89	100·13	99·80	
Sp. Gr. of small fragments by pycnometer at 60° F.	2·775	2·774	2·780	2·788	2·775	2·838	
†Whole of CO_2 calculated to $CaCO_3$	12·29	10·25	10·18	7·41	5·56	per cent.	

A trace of copper was detected in the ash slates. The oxides of chromium, vanadium, and zirconium were sought in a composite sample made up of equal weights of the powder of the five slates. Working on 10 grammes gave the annexed percentages:

Cr_2O_3 0·043; V_2O_3 0·048; ZrO_2 0·033.

These figures are slightly higher than we got for the Welsh slates, following precisely the same plan of operation. For the mean of the thirteen Welsh slates (see Part II., 'Phyllades of the Ardennes') the numbers were:

Cr_2O_3 0·003; V_2O_3 0·032; ZrO_2 0·022.

* Less than 1/100 per cent.

'*Phyllades of the Ardennes compared with the Slates of North Wales,' Part II.* (*Table* 3)

Description of Plate XXXIV

Fig. C. Best slate, Alexander Quarry, Moel Tryfaen, cut parallel to cleavage plane.

Fig. D. Best slate, Alexander Quarry, Moel Tryfaen, cut transverse to cleavage plane.

Fig. D′. Best slate, Alexander Quarry, Moel Tryfaen, enlarged: × 200.

(These are all No. 1 of Table 3.)

Fig. E. Slate, Llansantffraid Glyn Ceiriog, near Llangollen, cut transverse to cleavage (No. 2 of Table 3).

Fig. B. Slate, Llanrhwydroes, Anglesey, cut transverse to cleavage; bedding and cleavage coincident (No. 5 of Table 3).

Fig. A. Schist of Holy Island, Anglesey, cut transverse to folding (No. 10 of Table 3).

'*The Green Slates of the Lake District*' (*Table* 4)

Description of Plate XXXV

Fig. A.	Buttermere green slate, No. 3 of Table, cut transverse to cleavage plane	× 11
Fig. B.	Do. do. do.	× 84
Fig. C.	Penrhyn slate, No. 6 of Table in Part I. of 'Phyllades of the Ardennes,' &c., cut transverse to cleavage plane	× 11
Fig. D.	Do. do. do.	× 84
Fig. E.	Phyllade Devillien, No. 5 of Table, Part I. of 'Phyllades of the Ardennes,' cut transverse to cleavage plane	× 11
Fig. F.	Do. do. do.	× 84

Description of Plate XXXVI

Fig. G.	Buttermere green slate, No. 4 of Table, cut transverse to cleavage plane	× 11
Fig. H.	Do. do. do.	84
Fig. I.	Buttermere green slate, No. 4 of Table, cut parallel to cleavage plane	× 11
Fig. J.	Do. do. do.	· 84
Fig. K.	Tilberthwaite green slate, No. 5 of Table, cut transverse to cleavage plane	11
Fig. L.	Do. do. cut parallel to cleavage plane	· 11

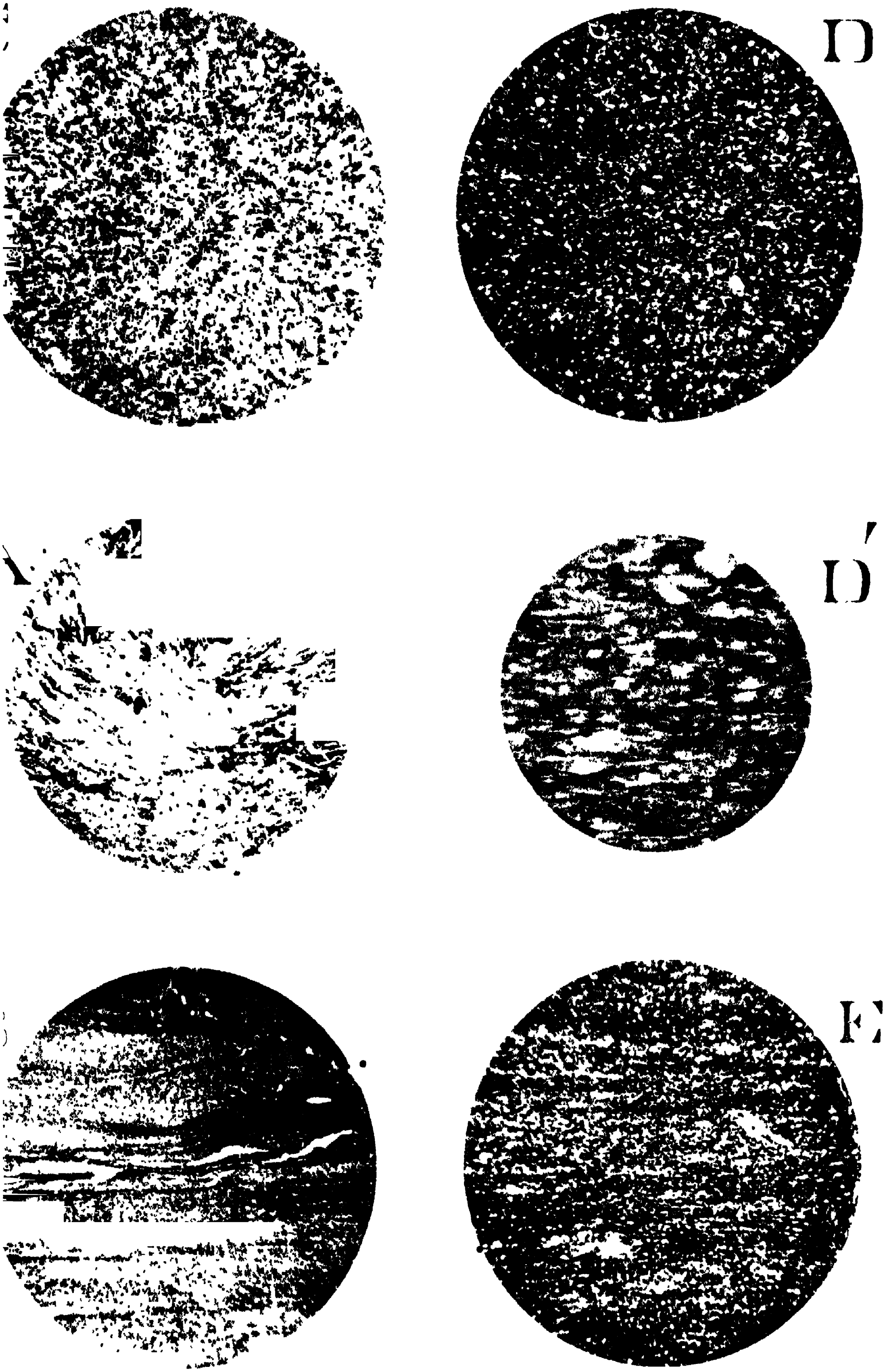
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PLATE XXXV.

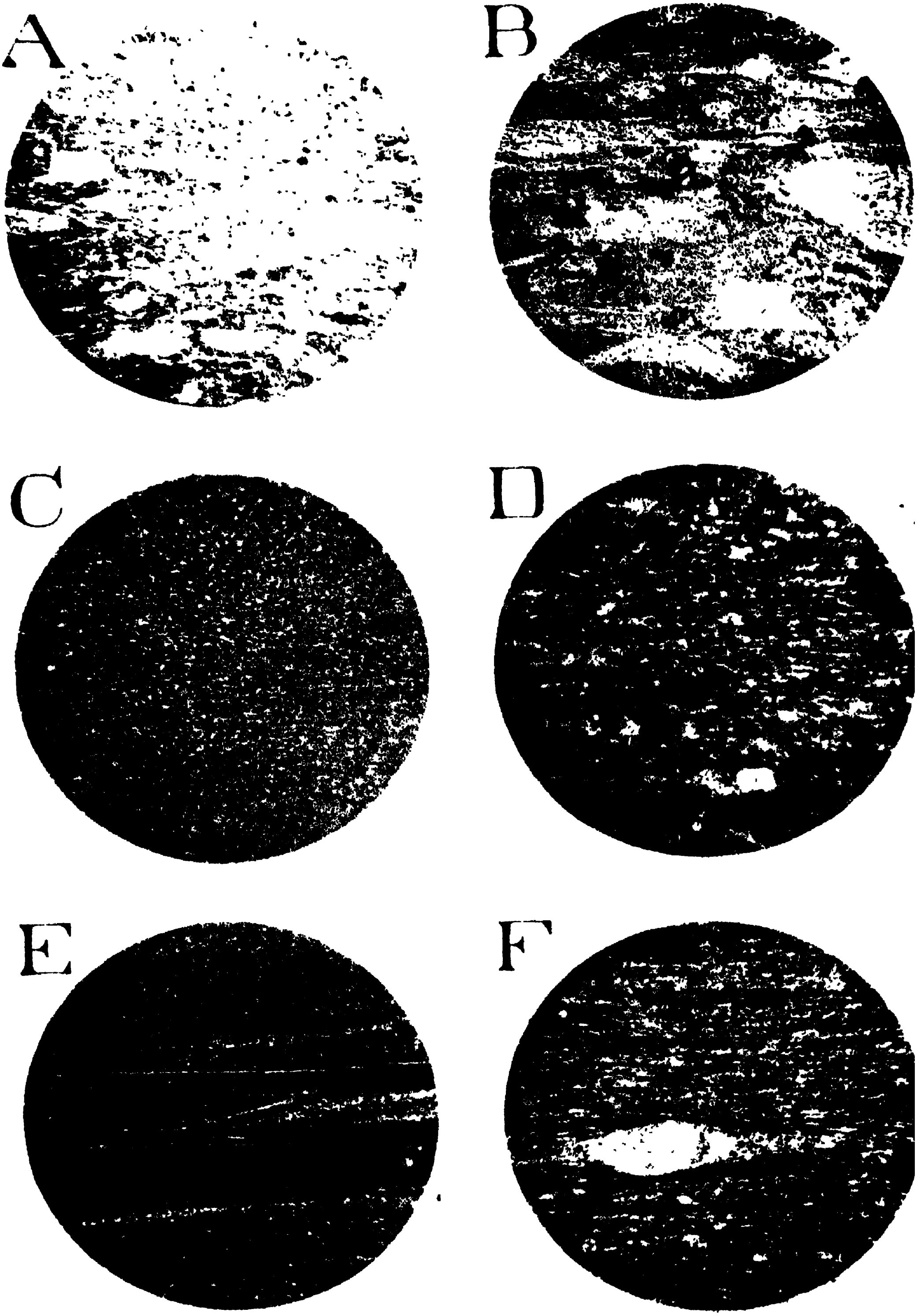
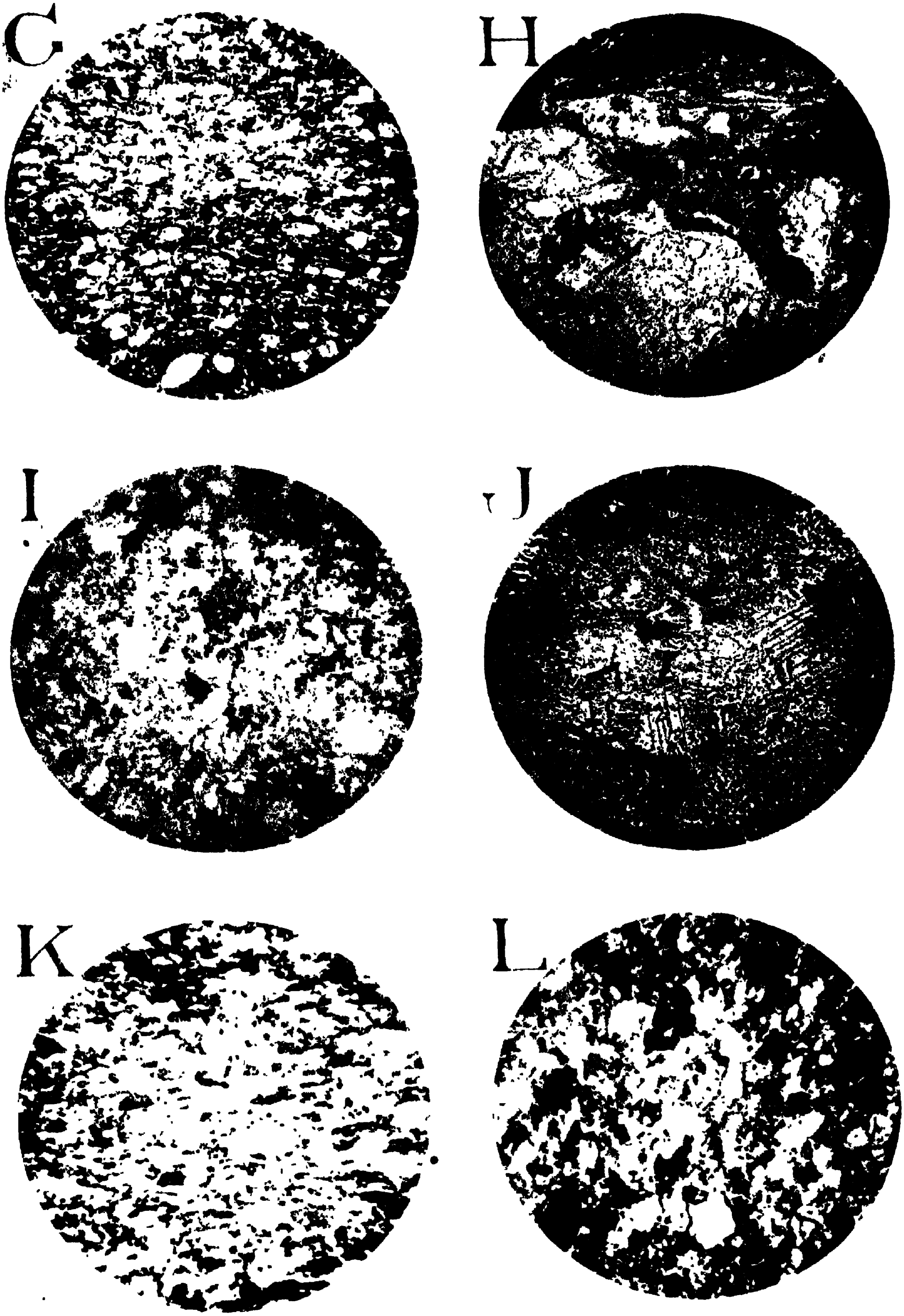


PLATE XXXVI.



BOOK III

REPRINTS, SPECULATIONS, AND CLOSING REMARKS

CHAPTER XX

DENUDATION OF THE TWO AMERICAS[1]

'Whence it appears, not only that in proportion as knowledge becomes quantitative do its previsions become complete as well as certain, but that until its assumption of a quantitative character it is necessarily confined to the most elementary relations.'—*Herbert Spencer's Essays*, vol. i. Essay iii.

INTRODUCTION

WHEN, in 1876, I had the honour to deliver a Presidential Address to this Society, I chose as its subject-matter 'Geological Time.' I then had the pleasure to lay before you some calculations relating to 'Chemical Denudation,' which at the time possessed some little novelty. Since the information was published it has, to a certain extent, been incorporated with geological literature. The subject was, however, far from being exhausted, nor is it likely to be for many years yet to come. In the meantime, having accumulated additional facts, it will be part of the object of this address to arrange and analyse them, so as to check the original generalisations, and further to illustrate the value in geological speculation of an accurate knowledge of the relative magnitude of the various objects and things dealt with.

The importance of a quantitative examination of geological data is not yet properly appreciated. Some of our authorities,

[1] Presidential Address to the Liverpool Geological Society, Session 1884–5.

as may be seen in quotations from them in manuals and text-books, hardly get further in their reasoning than is shown in the following examples. Suppose it were an attempt to estimate the extent of time during which the Coal-measures were being laid down, the formula would probably be as follows:—'So many feet of mud-sediment were laid down in a century; the Coal-measures are so many thousand feet in thickness; there are such a number of seams of coal of a given thickness, with a thousand years for the formation of each vertical yard, and the result arrived at is 640,000 years.' Or say it is an estimate of geological time that is required, then the following appears to be a not unusual method:—'Assuming that $\frac{1}{16}$ of an inch of sediment is laid down in one year, and the total thickness of the whole of the sedimentary rocks is so much, then the time taken in their production is 100,000,000 years;' or whatever figures this intricate calculation leads to. Whether the result arrived at is one hundred or one thousand millions, or only a million years, the figures are probably to most people equally incomprehensible, and therefore interesting. Nor need we be surprised, for there are some singular instances extant of inability to grasp the relations of figures when only thousands are dealt with; for instance, in Phillips's 'Treatise on Geology' (1839), in vol. ii. p. 8, there is a calculation relating to the sediment laid down by the Ganges, in which the average waste of the drainage basin of the Ganges is estimated at $\frac{1}{40000}$[1] of a yard, '*which is about* $\frac{1}{1111}$ *of an inch per annum* from the whole surface of the drainage.' The final result arrived at is that the whole of the English Tertiaries, averaging 300 feet thick, and 6,000 square miles in extent, could have been laid down by the Ganges in 8,000 years. The $\frac{1}{1111}$ and the time, in rough figures, 80,000 years

I was, however, very much interested when I met with this calculation, for, though it appears incorrect in most particulars, it is the first attempt I know of to measure the average waste of the drainage basin of a river. Previously Mr. A. Tylor had the credit of originating this mode of investigation ('Changes of the Sea Level,' 'Phil. Mag.' 1853), which was more fully worked out by Dr. Geikie ('Trans. Geo. Soc. of Glasgow, 1868'), and Dr. Croll in 'Geo. Time,' 'Phil. Mag.,' 1868).[2] If Phillips

[1] It is much more than this.

[2] See *Darwin on Earth Worms*, p. 233.

had been in the habit of making these calculations, he would have seen at a glance that the result was wrong, and would have checked his figures. To preserve accuracy it is, however, essential to have all calculations checked by another individual, for, as far as my experience goes, even mathematicians are no more exempt from errors of calculation than ordinary men.

The worst circumstance connected with these errors is, that they get copied from one book to another without any attempt at verification. Of this I could give numerous instances, but such a course would be invidious, and raise too great a storm about my ears.

It would seem, however, as if the figures possessed little meaning to most people, or the inaccuracies would be more quickly detected, at least by scientific men. For my own part, I can only say I shall feel indebted to any one who may find errors in my own calculations, if he will take the trouble to acquaint me with the fact.

With these preliminary remarks I will proceed to inquire in what way an accurate knowledge of the numerical proportions of the materials of rocks, their solid contents and extent in relation to one another, the earth and the ocean, and the rate of accumulation, may affect our entire conceptions of various geological problems.

In my presidential address, 1876–7, I ventured incidentally to remark that Hutton laid the foundations of our present knowledge of physical geology. The reviewer, in *Nature*, of my 'Chemical Denudation' (October 2, 1879), takes exception to this, and says: 'He gave us the grand method of geological study, but certainly many of the facts were well known before his time, and others have no relation to his researches or method.' Again, Darwin says: 'Until the last twenty or thirty years most geologists thought that the waves of the sea were the chief agents in the work of denudation; but we may now feel sure that air and rain, aided by streams and rivers, are much more powerful agents, that is, if we consider the whole area of the land.' This is true, but it is also true that Hutton had a correct conception of the same truth at the end of the last century; and it seems amazing that his cogent logic and reiterated statements to the same effect were so long in being appreciated and understood. A perusal of the 'Theory of the Earth' will well repay the reading even now, and I hope to see the time when a reprint, with full editorial notes, will be

offered to the public. At present it is all but inaccessible. The whole theory of subaërial denudation, as at present understood and accepted, is there as clearly laid down, and in as lucid and glowing language as is possible. How Lyell could call his style dry and uninteresting amazes me. To the last Lyell never thoroughly accepted the Huttonian theory of denudation, which underlies all our modern notions of physical geology almost to the extent to which gravitation underlies the science of astronomy. In the tenth edition, 1867, of his immortal 'Principles,' Lyell says (vol. i. p. 78), speaking of Hutton and Playfair: 'They ascribed valleys in general too exclusively to the action of the rivers now flowing in them, not allowing sufficiently for the excavating and transporting power which the waves of the ocean must exert on land during its emergence, nor for those inequalities of the surface which must be produced by movements accompanying the upheaval of the land;' but Hutton himself gives the clue to these misconceptions when he says: 'These consolidated masses are resolved in so slow a manner that *nothing but the most philosophical eye, by reasoning on a chain of facts*, is able to discover it'—the italics are mine. But, further, he indicates the way in which his ideas have finally triumphed: 'Nothing is more steady than the resolution of our land, nothing rests upon more certain principles, and there is nothing which in science may be more easily investigated.' Notwithstanding this luminous reasoning, the ideas were for a long time considered if not actually false yet exaggerated beyond all bounds of truth; but the demonstration has been complete, and through what?—*through quantitative calculations from ascertained data*, which put the truth of his generalisations beyond the shadow of a doubt. The amount of detritus brought down by rivers was weighed and gauged by many modern investigators, and found far to outweigh the denudation caused by the waves of the sea. Tylor, Geikie, and Croll calculated and proved it; Ramsay, by another line of reasoning, showed the enormous amount of rock that had been removed by sub-aërial denudation in the mountainous parts of Wales; and Mr. Whitaker followed it up by very original reasoning on the origin of escarpments. I venture to think that my own investigations of chemical denudation, a previously unworked mine, have also had some little influence in the acceptance and development of the theory. But we must not forget that

Hutton, though he could not prove it by figures, had as clear a conception of the fundamental truths of denudation as any that have come after him; and here lies his great merit, entitling him to be considered, in my opinion, the father of physical geology as now understood. I trust the preceding remarks have indicated, if but feebly, the value of figures in their bearing on the demonstration of geological truths; and I venture to say that no one can possess a competent knowledge of the forces which have fashioned our earth until he has realised in his mind, by numerical investigations, the proportions of the matter to be dealt with. There has been of late much crude speculation on the distribution of land and sea, the origin of continents, the permanence of oceans, &c., which, if the writers possessed an accurate knowledge of the geometry of the earth and ocean, we might have been spared.

Thus we have oceans deepened or shallowed at the will of the theoriser, without any attempt to follow out the consequences. It seems to escape these philosophers that the volume of water on the globe is a constant quantity, that if the oceans become deeper the land must increase in area, and if they become shallower the continents become submerged. In happy unconsciousness of these elementary truths they toil and spin to obtain some particular facts that have made an impression on their minds. I need hardly add that, unless the theorist grasps all the consequences of his hypothetical rearrangements of land and water, they possess but little value.

DENUDATION OF THE TWO AMERICAS.

My former calculations dealt almost exclusively with the amount of matter annually removed in solution in river water from the surface of England and Wales, and from some of the river basins of Europe. I now propose laying before you calculations of a similar nature relating to some of the larger rivers of the two Americas. This done, we shall be able to take a wider survey of the subject, and ascertain how the provisional generalisations led to by previous investigations are confirmed or otherwise by the greater experience since gained.

THE MISSISSIPPI

First, then, we will see what the Father of Waters, the Mississippi, tells us. I may observe that for a long while I found him very reserved and disinclined to answer to my questionings. Years elapsed, and letters innumerable were written, before I could alight upon any analysis of the waters of the Mississippi, reliable or otherwise. At last, through the kindness of Professor J. W. Spencer, of the State University of Missouri, I was supplied with the following analysis:—

ANALYSIS OF MISSISSIPPI WATER, NEAR CARROLTON, A FEW MILES ABOVE NEW ORLEANS[1]

In a Gallon (56,000 grains)

	Grains
Potash sulphate, „ chloride, Calcium chloride	3·154
Silicic acid	2·455
Alumina	1·753
Calcium carbonate, Magnesium	7·307
Organic matter	0·818
Total solid residue	15·487

According to this analysis the proportion of total solids in solution is by weight $\frac{1}{3615}$. If we take the mean annual discharge of the Mississippi at 541,666,666,666 tons,[2] in round figures, there are 150 million tons of solids in solution per annum poured into the Gulf of Mexico by the Mississippi—a truly remarkable quantity, which, if reduced to rock at 15 feet to the ton, is represented in round numbers by 80 square miles 1 foot thick. According to Messrs. Humphreys and Abbot, the proportion of sedimentary matter to the water by weight is $\frac{1}{1500}$,

[1] Avequin (*Journ. Pharm.* 3[3] *XXXII.* p. 258, 1857).

[2] 'Report of Humphreys and Abbot' (1876), p. 146 19,500,000,000,000. cubic feet, at 36 feet to the ton.

The notes in italics are not part of the original Address.

Note, July 1903. *According to an analysis by Professor J. A. Dodge (Geology of Minnesota. Vol. II of the final Report by N. H. Winchell,* 1882-5), *the grains per gallon in the Mississippi above Minneapolis were* 12.392, *and below* 12,322. *The gallons, I infer, were United States gallons* (56,000 *grains*).

and the total discharge of matters in suspension, excluding the three outlet bayous, is, according to them, 362,723,214 tons.[1]

The amount of matters in solution varies within certain limits in river water, according to the time the samples are taken. There are in some rivers—the Nile, for instance—seasonal variations; and doubtless a river with many affluents traversing strata of various degrees of solubility must vary in the chemical composition of its waters according as the flood may come from one or the other tributary basin. The last analysis would make the total solids in solution exactly $\frac{1}{3}$ of those in suspension, and it is a remarkable fact that this is the proportion that holds good with the Danube and the Nile, as I have before pointed out.[2]

If we take the drainage area of the Mississippi proper at 1,244,000 square miles, the calculated amount of solids in solution, according to the analysis, will be 120 tons removed from each square mile of surface per annum. From the surface of England and Wales I have shown that 143·5 tons per annum are removed in solution,[3] and from the Danube basin 90 tons, so this is a mean, and probably correct.

It has been estimated that the basin of the Mississippi is lowered at the rate of one foot in 6,000 years,[4] but this rate has been calculated from the removal of sediment alone;[5] if we add to the matter removed mechanically that in solution, it will raise the rate to one foot in 4,500 years.[6] What stronger evidence can we have of the importance of chemical action in geological investigation—an importance that has hitherto been strangely overlooked?

Not less surprising, considering the apparent insolubility of silica by ordinary agencies,[7] is the fact that in round numbers

[1] 812,500,000,000 lbs. [2] 'Rivers.' *Trans. L'pool Geo. Assoc.*, 1882.

[3] *Chemical Denudation*, p. 20. [4] Geikie, *Text Book of Geology*, p. 444.

[5] According to the figures I have taken, it would be one foot in 6,375 years.

[6] This is estimated as follows: Drainage area 1,244,000 square miles; annual sedimentary discharge from the same area 362,723,214 tons, solids in solution 150,000,000 tons; the average rock is estimated at 15 feet to the ton. Strictly speaking, to this should be added 750,000,000 cubic feet of matter, estimated to be pushed along the bottom and the discharge from the bayous. For simplicity's sake I omit these elements.

[7] Mr. M. E. Wadsworth has shown that ordinary atmospheric agencies produce a greater effect upon rocks of a siliceous character than is generally believed.—See *American Journal of Science*, December, 1884, p. 466.

from 23 to 24 millions of tons of silica are poured into the sea annually by this river, while there are 70 million tons of carbonate of lime and magnesia.

There is also an exceptional quantity of alumina and a low percentage of sulphates in this water.[1]

THE RIVER PLATE, OR RIO DE LA PLATA

The next river I shall deal with is the Rio de la Plata, the second greatest river of the South American continent. I am indebted to the very exhaustive series of observations and analyses of the waters of this river contained in the report to the Commission of Running Waters of the City of Buenos Ayres, by Juan J. J. Kyle, in 1872 and 1874, for most of the information relating to this river. I must here express my thanks to Mr. J. E. Hawkes for his valuable assistance in translating the pamphlet for me.

I find that the mean of 14 analyses of water taken at different times (April, May, and June) in the neighbourhood of and above the city of Buenos Ayres gives a proportion $\frac{1}{6441}$ of solids in solution, which, taking the dry weather flow of the La Plata at 670,000 cubic feet per second[2] (Bateman), will equal 2·8886 tons per second, or 91 million tons per annum in round figures. The *dry* weather flow of the La Plata equals the *mean* annual flow of the Mississippi. The mean annual flow of the La Plata is not known, but it must be greatly in excess of the dry weather flow, and sufficient to bring up the total amount of dissolved matter to above that of the Mississippi, though it appears from the analyses to have a less percentage in its waters than has the Mississippi. It seems from the report of 1874 that in two analyses of the La Plata water on September 15 and 18, the matter in solution reached a proportion of $\frac{1}{3125}$. According to an analysis of the waters of the Parana supplied me by Dr. Frankland,[3] they contained a proportion of only $\frac{1}{9926}$ of solids in solution. Mr. Juan Kyle states that there is very little difference between the waters of the La Plata taken at 850 metres from the shore and the water

[1] See *Rivers of North America* for other information carefully arranged by Israel Russell and Dr. H. J. von Hoesen.

[2] See *Chemical Denudation*, p. 55.

[3] *Ibid.* p. 23. 10·08 parts per 100,000.

of the Paraná de los Palmas. As the Parana supplied, according to careful measurement by Mr. Bateman, 520,000 cubic feet of water per second to the La Plata, while the Uruguay was estimated at only 150,000 cubic feet at the same time, it follows that the chemical constituents of the water of the La Plata must vary considerably at different times and seasons. Probably the analysis on which I have made my calculations will represent a fair annual mean of the solids in solution.

The estimated drainage area of these two rivers is 1,250,000 square miles, so that were the mean annual discharge known it would probably turn out that the greater discharge of the La Plata would more than compensate for the smaller percentage of dissolved matter in its waters, and bring the chemical denudation per square mile of river basin up to or beyond that of the Mississippi.

The observations of Mr. Bateman were taken in the month of December 1870, when the river was at its lowest state, 'a continuous drought of six or seven months having diminished the ordinary sources of supply, and the periodical rise from the Andes not having commenced.' It is difficult .nay impossible to predict the mean delivery from the dry weather flow; for instance, the mean flow of the Rhine is given by Beardmore as over twice the ordinary summer flow, that of the Rhone at Avignon as nearly three times, and the Nile at Cairo as over seven times.

The waters of the La Plata are distinguished by the fineness of the matter held in suspension; this consists, according to Mr. Kyle, principally of clay. This clay continues a long time in suspension, even after filtering. It will pass through the pores of the best filtering papers, the water preserving its turbidity even after months of repose. This is a feature, according to Mr. Kyle, which is common to all waters that are weakly alkaline. Several chemicals added to the water will, however, precipitate the solid matter by making the muddy particles coagulate into larger compound particles. Chloride of calcium in the proportion of 1 to 5,000 parts will act in this manner. The analyses given by Mr. Kyle are after 48 hours' subsidence. The matters in suspension, as is the case with other rivers, vary much according to the state of the river, and the water is more impure near the shore than at 850 metres distant.

It is pretty well known that an admixture of sea-water with turbid fresh water tends to hasten the precipitation of the solid

matters,[1] but it is very probable, as will be seen before I conclude, that the extremely divided solid matter will be carried far and wide by oceanic currents before it can settle to the bottom.

The annual amount of solids in suspension in the La Plata waters has never to my knowledge been determined, or even approximately estimated.

THE ST. LAWRENCE

The next river on the American continent about which we have any knowledge worth speaking of is the St. Lawrence. The elements for a calculation such as I wish to make are, however, unfortunately rather vague. Even the area of its basin is stated differently by different authors. According to Guyot[2] its basin—including, I presume, the area of its immense lakes—is two-fifths that of the Mississippi, while it is said to pour into the sea more than twice its volume. This must, however, be an error, for it would give 40 inches of rain run off the area per annum; whereas, according to the Rainfall Map of the World prepared by Loomis ('American Journal of Science,' vol. xxv. p. 88, January 1883), the whole basin lies in the area of rainfall of from 25 to 50 inches. If we were to take it at 20 inches run off the ground per annum, or half the stated delivery—that is, a volume equal to the Mississippi—after deducting the area of the great lakes, where denudation cannot act, the chemical denudation would still be enormously great.

The only analysis I have met with gives the proportion of solids in solution at $\frac{1}{6230}$,[3] so that the denudation would amount at that rate to over 200 tons per square mile per annum. The one thing probable, however, is that the matter removed in solution is more per square mile than from that of

[1] See 'Precipitation of Clay in Fresh and Salt Water,' by D. Robertson (*Trans. of Glas. Geo. Soc.*, vol. iv. part iii., p. 257).

[2] *Physical Geography*.

[3] 16·05 per 100,000 parts. *Jahresbericht der Chemie*, per Professor Frankland.

Note, July 1903.—*This analysis was, I believe, made by Sterry Hunt, and published in the 'Geology of Canada,'* 1863, *p.* 567 (*South side Point de Cascades*).

Note, July 1903.—*After this Address was delivered I obtained a sample of the St. Lawrence water through the late Mr. James R. Montgomery, a steamship owner of Liverpool. It was analysed by the late Mr. Norman*

the Mississippi basin.[1] The matter removed to the sea in suspension must be comparatively small from the clearness of the water due to its passing through the great lakes.

Tate. The label on the bottle was as follows: 'Sample of water taken from the south side of the river opposite Montreal, November 24, 1884.'

Analysis

In 100,000 *parts of water*

Free Ammonia	0·001
Albumenoid	0·008
Nitrogen as Nitrates	0·003
Chlorine in Chlorides	0·355
Oxygen absorbed in 15 minutes at 80°	0·022
,, ,, *4 hours*	0·047
Carbonate of Lime	7·350
Carbonate of Magnesia	2·270
Sulphate of Lime	0·400
Sulphate of Magnesia	1·512
Sulphate of Soda	0·236
Chloride of Sodium	0·585
Carbonate of Soda	0·954
Silica	1·480
	15·223

[1] Through the kindness of the late Dr. Alfred Selwyn, Director of the Geological Survey of Canada, I was, after the above was written, supplied with the following information obtained from the Montreal Harbour Commissioners' Engineer:

'The discharge of the St. Lawrence River opposite Victoria Pier, Montreal, varies from about 580,000 cubic feet per second at high water of 24 feet on the lower lock sill of the Lachine Canal in the latter part of May, to 330,000 at low water of 17 feet on the sill in October.'

Thus it would appear that the minimum flow at Montreal is somewhat less than the mean flow of the Mississippi. The difference between the maximum and minimum flow is probably less than that of any other great river on the globe, due, doubtless, to the enormous reservoirs, in the form of that wonderful group of lakes from which its supplies are drawn. As the St. Lawrence receives the waters of the Richelieu, the St. Maurice, the Saguenay, and many minor rivers below Montreal, the estimate of the discharge on which I have based the calculations is probably near the mark, and is a satisfactory proof of the accuracy of the reasoning adopted.

NOTE ON NIAGARA, &c.—The *English Cyclopædia*, article 'Canada,' says that the mass of water projected over the Falls of the Niagara per minute is 710,000 tons (= 426,000 cubic feet per second), but at what season is not stated. Stanford's *Compendium of Geography and Travel* gives it as 169,344,000 gallons per minute (= 756,000 tons; in the 'General Description of the North American Continent,' &c., in page 356, Part II., at 701,250

THE AMAZONS

The River Amazons is compared by Agassiz in its main features to the Mississippi, inasmuch as it lies in a Cretaceous basin.[1] I think, however, the analogy is a fanciful one. The valley of the Amazons is distinguished from other river valleys by its immense extent, the drainage basin being estimated by Humboldt at over three million square miles. The basin appears to have existed much in its present form before it became partially filled with the remarkable deposits of red sandstones and clays which cover an immense area, and which the river is now engaged in rapidly removing to the sea. The upland portions of the basin are largely composed of the granitic

tons). It also states that the River St. Lawrence is the largest in North America as to volume (p. 351). The *Popular Cyclopædia* says it discharges, it is computed, 100,000,000 tons of water each hour (= in round figures 1,600,000 tons per minute); while Reclus (*The Earth*, sect. i. p. 344) says the river above the cataract discharges on an average 1,300 to 1,400 cubic yards of water per second (= 63,000 tons per minute). The *Handbook and Official Catalogue*, Paris Universal Exhibition, 1878, p. 14, says: 'The calculated discharge from the upper lakes by the Niagara River is over twenty millions of cubic feet per minute' (= 555,555 tons per minute) about half the discharge of the Mississippi. The statements of the areas of the basin are equally discrepant. According to the *Imperial Gazetteer* the area of the basin of the St. Lawrence is 297,600 square miles, of which 94,000 are covered with the water of the lakes alone. The *English Cyclopædia* says: 'The whole basin of the St. Lawrence is calculated by Darby to contain 537,000 square miles, 149,000 of which is occupied by lakes and its estuary, and that the basin above Niagara is 250,000 square miles.' Stanford's *North America* does not give the area of the St. Lawrence basin, but says that the Mackenzie and its tributaries are about 550,000 square miles, almost double that of the St. Lawrence basin.

Note, July 1903.—*Schermerhorn, in the 'American Journal of Science'* (*April* 1887, *pp.* 278–84), *gives full particulars of the size, depth, and outflows of the great American lakes. In a paper read before the American Society of Engineers, Mr. Benjamin Rhodes gives the average flow of the River Niagara, according to very careful measurements of the U. S. Lake Survey, as* 275,000 *cubic feet per second.* ('*Engineer,' November* 27, 1885, *p.* 417.)

Discharge of the St. Lawrence.—Forty miles below Montreal the area of the cross section in 1886 *was* 115,300 *square feet, the discharge* 311,100 *cubic feet per second, and in* 1895, *with water level* 1 *ft.* 9 *in. lower, the cross section area was* 105,400 *square feet, and discharge* 216,600 *cubic feet per second.* (*McLeod, 'Min. Proc. of Institution of Civil Engineers,' vol. cxxviii., May* 1897, *p.* 376.)

[1] *Geological Sketches—Physical History of the Valley of the Amazons*, p. 171.

and crystalline rocks which are such a prominent feature in the Brazils. The sandstones and clays that have so large a development over the bottom of the basin appear to be Post-tertiary, and laid down by the river itself. There are, however, Tertiary rocks in a part of the basin,[1] possessing an estuarine character, in addition to Cretaceous rocks; while on the flanks of the Andes draining into the river are found both Cretaceous and Carboniferous rocks. The larger area of the basin appears, however, to be occupied by crystalline rocks and the Post-tertiary sandstones and clays; but a very large part of the basin seems never to have been geologically explored. The basin of the Amazons has also the peculiarity of being situated both to the north and south of the equator, and in an area of very heavy rainfall. The chart of mean annual rainfall by Loomis, before referred to, puts it at from 50 to 75 inches for about ⅘ of its area, the remainder near the Andes being over 75 inches. The volume of water discharged by the river has been estimated at from 2,700,000 to 3,510,000 cubic feet per second. Taking the mean, this would give about 15 inches run off the ground, or 0·25 of the total rainfall if we take it at 60 inches, about the proportion that flows off the Mississippi basin. The mean rainfall of the Mississippi basin is estimated by Messrs. Humphreys and Abbot at 30·4 inches. Probably 60 inches would represent the mean rainfall of the Amazons basin. For the purposes of this calculation I take the mean discharge at 3,105,000 cubic feet per second, or 86,250 tons = 2,719,980,000,000 tons per annum.[2]

Through the kindness of Mr. E. Edmondson, of Messrs. Gunston & Co., of Liverpool, I have obtained a sample of the water of the Amazons, taken in mid-stream between the

[1] See 'On the Tertiary Deposits on the Solimoes and Javary Rivers in Brazil,' by C. Barrington Brown, *Q.J.G.S.*, 1879, also 'Ancient River Deposits of the Amazons,' *ibid.*

[2] Bates, *Naturalist on the Amazons*, vol. i. p. 237, says: Von Martius estimates the volume of water passing through the Straits of Obydos at 499,584 cubic feet per second. He arrives at this result by taking the depth in the middle at 60 fathoms, and at the sides 20 fathoms, the width being given as 1,738 yards. Suspecting some error—as the volume of the La Plata in dry weather exceeds this estimated volume of the Amazons—I have re-calculated the delivery from these elements and find that it cannot be less than 3,000,000 cubic feet per second, but may be more according to the form of the bottom. Our gratitude is due to those who give us the means of checking their results.

Narrows and Santarem in June of this year. This sample I submitted to Dr. Percy F. Frankland, and the following is his analysis:—

PARTS IN 100,000

Silica	0·98
Iron and alumina	0·38
Carbonate of lime	2·75
Carbonate of magnesia	0·22
Sulphate of magnesia	0·37
Chlorate of potassium	0·23
Chlorate of sodium	0·15
Sulphate of soda	0·13
Organic matter	0·71
Total solids in solution	5·92

This gives a proportion of total solids in solution of $\frac{1}{16890}$, or = 5·1 tons per second.

The total delivery of matters in solution will amount according to these data to 160,833,600 tons per annum, or, if we estimate the basin at three million square miles, to 50 tons per square mile per annum.

It will be observed that the total amount of solids in solution delivered to the sea is not much greater than that we arrived at for the Mississippi. This is a fact worth knowing, and due doubtless to the preponderance of gneissic rocks, sandstones, and clays of an insoluble character. It is also worth noting that the proportion of silica to the total matter in solution corresponds very closely with that of the Mississippi, amounting to 26,624,481 tons per annum.

It is also evident that the rocks and Pampean deposits[1] occupying the basin of the La Plata, are also of a more calcareous and soluble character than the Amazonian rocks.

Not less interesting is it that the carbonate of lime, roughly speaking, is one half of the whole of the solids in solution.[2]

It follows from these data that the matter removed in

[1] See *Geological Observations* (Darwin), second edition, pp. 313-69.

[2] See *Chemical Denudation*, p. 24.

Note, July 1903. *Dr. F. Katzer gives the total solids in solution at a depth of* 0·5 *metre and* 25 *metres as* 0·056 *and* 0·039 *gramme per litre* (= *respectively* 5·6 *and* 3·9 *parts in* 100,000). *Suspended matter from* 3 *to* 4 *times as much.*—*Nature, August* 25, 1898, *vol. lviii. p.* 399.

suspension must bear an excessive proportion to that in solution as compared with other rivers. The deposits forming the banks of the river are of a loose and friable nature, on which the river makes great inroads. The proportion of matter in suspension has never to my knowledge been estimated.

Bates, comparing the Pará and the main Amazons, says: 'In the former, the flow of the tide always creates a strong current upwards, whilst in the Amazons the turbid flow of the mighty stream overpowers all tides, and produces a constant downward current. The colour of the water is different, that of the Pará being of a dingy orange brown, whilst the Amazons has an ochreous or yellowish clay in it.' Also: 'Indeed the fresh water tinges the sea along the shores of Guiana to a distance of nearly 200 miles from the mouth of the river.' [1]

INFERENCES AND GENERALISATIONS

In my former address I said: 'Taking into consideration what we know of the geology of the world, I think we have sufficient grounds for a provisional assumption that about 100 tons of rocky matter is dissolved by rain per English square mile per annum.' [2] This, at the time, was considered a very bold statement; but from the data I have laid before you respecting the American continents, I venture to think it will now be considered, as applied to the whole world, a very fair approximation.

Let us pause to consider the meaning of all these figures, for unless they have a meaning which the mind and imagination can seize upon, the wearisome labour of collecting the data and making the computations were wellnigh wasted.

First, as regards the Mississippi, of which we possess the most reliable particulars. I have shown that the estimate of the rate of denudation of its basin must be increased in round figures from $\frac{1}{6000}$ to $\frac{1}{4500}$ of a foot per annum [3] in consequence of the solid matter which is removed in solution.

[1] *Naturalist on the Amazons*, vol. i. p. 5.

[2] *Chemical Denudation*, p. 24.

[3] This calculation, as before explained, takes no account of matter pushed along the bottom; its quality has not been determined with much accuracy, and it is probable, as Mr. Tylor has suggested, there is more than has been estimated. This would further reduce the time.

Is it not a striking instance of the little importance attached to chemical denudation as a geological agent, when the matter removed in solution does not enter as an element into the calculations of such observant reasoners as a Geikie or a Croll? Thus we arrive at the first and not unimportant result which I promised from quantitative examination. Now mark, it is not that geologists were unaware of the effect of chemical action on the rocks. Take up any text-book or manual, and you will find a chapter devoted to it and the whole process correctly explained; nevertheless, the quantity of matter removed was not realised, and never could have been except through laborious calculations. That being done, it is easy to see how these great results occur. Examine the hardest rock, and you will find it weathered; you will find it coated over with a crust of a thickness varying with the time its surface has been exposed. This crust is composed of the constituents of the rock that remain after part have been removed by chemical action.[1] Examine the waste talus from some of the old quarries at Penmaenmawr, and you will see that atmospheric agents have, in the space of 30 years, perceptibly affected a felstone rock that seems at first sight absolutely indestructible.[2] How much more, then, must they affect rocks of a more friable and soluble nature! I have shown that $\frac{1}{12978}$ of a foot per annum is removed from the surface of England and Wales in a soluble form every year,[3] say $\frac{1}{1081}$ of an inch, so that in 30 years it would amount to $\frac{1}{36}$ of an inch. This is the *mean* denudation; but I have also shown that the denudation is very much equalised by the fact of the harder rocks usually occurring in areas of great rainfall.[4]

It is therefore not unlikely that if we were to institute accurate experiments over a sufficiently long time, it would turn out that the calculation of the amount of matter removed in solution could be verified by direct tests, and that even

[1] It is usual to refer this action to the carbonic acid present in the rain water, but Mr. Alexis A. Julien has brought forward a great body of facts to prove that the solvents of the rocks are largely organic acids existing in decaying vegetable matter.—'On the Geological Action of the Humus Acids' (*Proc. of the American Assoc. for the Advancement of Science*, Saratoga meeting, 1879).

[2] This stone is largely used for making 'setts' for street paving, sold under the name of 'Welsh granite setts,' and found to be the most lasting material for the purpose.

[3] *Chemical Denudation.* [4] *Ibid.*

these hard rocks would be found to waste at something near the indicated rate.

It would appear from the examples of the Mississippi, the Nile, and Danube, that the matter brought down in solution and in suspension is as 1 to 3. These examples are of rivers where there have been the most accurate and fullest data to judge by. Whether the proportion would be borne out in other river basins we have no very good means of judging; but it would appear that in large rivers the nature of the rocks is so varied, the areas being so extensive, that the relation of the materials in solution to those in suspension has a tendency to keep very constant. It will be seen from a consideration of these facts, that matters chemically dissolved in the water must play a much more important *rôle* in the reconstruction of the earth than was formerly suspected.[1] What becomes of all these mineral matters ceaselessly flowing into the sea? It has been shown by Mr. Buchanan [2] that the proportions of mineral substances to each other in sea water are nearly constant everywhere, although there is a variation in different seas in the proportion of total mineral matter in solution to the water it is dissolved in. Nature has achieved a balance of supply and demand. It is also well known that the coarser materials in suspension, unless brought under the influence of a strong current, settle near the mouths of the rivers, and then spread themselves, by help of tides and winds, along the coasts, and there mingle with the detritus the sea wears away from the coast. The finer particles distribute themselves over a larger area, and, probably, the very finest over the whole sea-bottom. In every ocean dredging there is a greater or less amount of argillaceous matter whether it be in the ooze or the Red Clay - which I suggest is more likely to be 'the dust of continents' than to arise from the disintegration of volcanic matter such as pumice, but it is no doubt largely mingled with such volcanic materials, as Mr. Murray clearly shows. It seems to me rather a far-fetched notion that the winds should contribute dust to the deepest ocean, but that the waters should make no mechanical contribution to the deposit. The bulk of the ocean water is so great as compared with the probable amount of matter in a state

[1] It is singular that Hutton, in his *Theory of the Earth*, estimates, 'at a gross computation,' that one-fourth of the solid land is composed of matter which had formed the calcareous tests of animals.

[2] '*Challenger*' *Reports*.

of the finest comminution that can get into it, that it might not even be possible to detect the presence of substances in suspension in a sample of ocean water. At the same time the water might contain quite sufficient to account for much of the argillaceous matter found in the deep ocean soundings. I have shown that the matter in solution in river waters is, roughly speaking, one quarter of the whole matter in solution and suspension. The finest particles—sufficiently fine to be carried away by oceanic surface currents such as the Gulf Stream—are probably not in aggregate bulk half as much as the matter in solution. If we take as an example the estimate I have given of the chemical denudation of England and Wales, it will amount, as I have already shown, to $\frac{1}{36}$ of an inch in 30 years.

This would give, supposing the impalpable mud to be worn off at half that rate, 60 years for the denudation of $\frac{1}{36}$ of an inch. The area of the sea to land is roughly as 3 to 1, therefore at this rate it would take 180 years for $\frac{1}{36}$ of an inch of mud converted into rock to accumulate if distributed evenly over the ocean floor. When we consider that the average depth of the ocean is over 2 miles, $\frac{1}{36}$ of an inch distributed through it would amount to no more than about one five-millionth part, and this, be it remembered, has 180 years in which to accumulate and settle; so that if we give each particle of these fine substances in suspension 10 years to settle to the bottom, there would never be in the ocean water at any one time more than one ninety-millionth part of matter in suspension, an amount so small as to be practically imperceptible. The probability that such an infinitesimal amount of matter in suspension may be present is still more evident when we find that fine sand floats on the surface of the sea for considerable distances; for Professor Verrill says that in the course of the Gulf Stream they always take with their towing nets more or less fine, siliceous sand[1]

[1] Professor A. E. Verrill also says 'that in the Gulf Stream slope examined by us, the bottom in 70 to 300 fathoms, 60 to 120 miles from the shore, is composed mainly of very fine sand, largely quartz, with grains of felspar, mica, magnetite, &c.; with it there is always a considerable percentage of shells, of foraminifera, and other calcareous organisms, and also spherical, rod-like and stellate sand-covered rhizopods, often in large quantities. In the deeper localities there is usually more or less genuine mud or clay, but this is often almost entirely absent, even in 300 to 500 fathoms. The sand, however, is often so fine as to resemble mud, and is frequently so reported when the preliminary soundings are made and recorded.' 'The prevalence of fine sand along the Gulf Stream slope in this region, and the remarkable

('Amer. Jour. of Science, 1882,' xxiv. p. 449). I think it is fairly evident, from the foregoing calculations, that there may be accumulations going on in the great oceans which we can no more see than we can the matters in solution.[1] It is only because the mineral substances get concentrated in the sea water that they are forced upon our notice. They slowly concentrate until a balance is attained, when they are removed from the sea water at the same rate that they are poured into it. How are the millions and millions of tons annually supplied by the land to the sea ultimately disposed of? But let us first make a rough approximation to the amount of matter in solution annually poured into the Atlantic.

EFFECT OF SUBSTANCES IN SOLUTION ON THE ATLANTIC OCEAN

The basin of the Atlantic, it has been estimated, contains in Europe three millions of square miles, nearly half a million in Asia, and about six millions in Africa, not less than six millions in South America, and more than six millions of square miles in North America, in all about twenty-one millions of square miles.[2]

If we estimate this area as yielding 100 tons per square mile per annum, which I have shown is a moderate computation[3] further verified by the remarkable fact that the mean of

absence of actual mud or clay deposits, indicate that there is here, at the bottom, sufficient current to prevent, for the most part, the deposition of fine argillaceous sediments over the upper portion of the slope in 65 to 150 fathoms. Such materials are probably carried along till they eventually sink into the greater depths nearer the base of the slope, or beyond in the ocean basin itself, where the currents are less active.'—*Amer. Journ. of Science*, 1882, vol. xxiv. pp. 148–9.

[1] Mr. Thomas Higgin, F.L.S., of Anderton Salt Works, Northwich, prepares his finest quality of salt by precipitating the slight proportion of muddy impurities which the cold brine holds in suspension, by heating it to a temperature of 107° Fahr. in large vats. At my suggestion he carried out a series of experiments to determine the proportion of mud so removed. He found that it amounted to 57 lbs. per 1.556 tons of brine = $\frac{1}{61114}$. Looking at the brine purified and unpurified together, in clean bottles, the difference between them is so faint as to be hardly distinguishable. It of course affects the colour of the manufactured salt to a much greater extent, both by the higher proportion the impurities bear to the salt, and the whiteness of the salt.

[2] *English Cyclopædia*, article 'Atlantic Ocean.'

[3] *Chemical Denudation*, p. 24.

the four great American rivers I have given amounts almost exactly to 100 tons per square mile, the total amount of mineral matter poured into the Atlantic in solution every year amounts to 2,100 million tons per annum. We may represent this astounding result in a way to be readily realised by roughly placing it as rock at half a cubic yard to the ton or equal to a specific gravity of 2·67, in which case the deposit would cover 1,016 square miles one foot thick annually; or, to put it in another way, in less than six years one cubic mile of solid rock of a specific gravity of 2·67 is dissolved and carried into the ocean by the rivers draining the Atlantic basin.

How is this enormous amount of dissolved mineral matter disposed of? It has been said, I think more from a keen desire to establish a pet hypothesis than from any basis of fact, that the greater part of the carbonate of lime is used up in the formation of coral reefs, or on the shore by molluscs. It is indeed extraordinary, in face of the vast areas of calcareous ooze discovered by the *Porcupine* and *Challenger* soundings, that such a contention should be seriously maintained. A little consideration will show, however, that one foot of calcareous matter distributed over an area of the Atlantic Ocean equal to the land from which it is derived—viz. 21 millions of square miles—would equal in round numbers 4,000 cubic miles. Now this amount of matter is directly supplied by the rivers to the sea, in the form of carbonate of lime, at the rate of 50 tons per square mile per annum, in about 38,000 years.[1] Are there 4,000 cubic miles of coral reefs in the Atlantic? Is it not preposterous to contend that nearly all this enormous amount of calcareous matter is used up on the shores, when we have direct evidence to the contrary?[2] But if we were to grant that the lime is disposed of on the littoral zone, what becomes of the remainder of the mineral substances?—of the 20 million tons of silica, for instance, that I have shown is poured into

[1] See *Chemical Denudation*, p. 37.

[2] A large part of the coral deposits in the Pacific Ocean is being formed on and around islands, which are all assumed by Mr. Wallace to be volcanic, and where there has never, according to him, been continental land. How can corals in such a position help to build up continents that have remained permanently elsewhere—*i.e.* in their present positions? If not, the carbonate of lime of which the islands are composed must, on his favourite hypothesis, be as entirely lost to the land as if it had been deposited as Globigerinæ ooze at the bottom of an abysmal ocean from the dawn of creation.

the Gulf of Mexico by the Mississippi alone? Is this all used up on the shore, and does none get out into the deep ocean?

If, then, as these calculations clearly show, a large part (the largest part, as I believe) is used up by pelagic organisms and finds its way to the ocean bottoms—the whole central bank of the Atlantic is covered with the tests of these animals—it will be quite evident on a little consideration that the constitution of the rocks of the globe would be undergoing a gradual alteration had Nature no means of redistributing these oceanic deposits; and the only means we are acquainted with is by upheaval into land and redistribution by denudation.

It is a self-evident proposition that if a large portion of the carbonate of lime of the lands of the globe is being annually abstracted from them and deposited in the abysses of the ocean, from which it is never recovered, the total amount of lime in the rocks that compose the land must have been diminishing from the dawn of geological time.[1] But we find this is not so, for the rivers draining basins in which the younger rocks predominate bring down more lime in solution than do those from palæozoic areas.

I trust I have now made it pretty plain that measurement and proportion are things not to be neglected in Geology; but hitherto they have been, comparatively speaking. Vague and loose theories are given to the world which, if tested in the way I have pointed out, quickly fall to pieces. The method of treating great problems is often excessively crude and imaginative, founded on mere analogy, than which nothing is more often misleading. Much of the weak theory not seldom incorporated with present-day geology would vanish if juster conceptions of the proportions and relative magnitude of the things treated of were general. It would almost seem as if some theorisers entertained the idea that the lesser *may* include the greater.

FURTHER EXAMPLES OF THE BEARING OF MEASUREMENT AND PROPORTION ON GEOLOGICAL PROBLEMS

This address was commenced with certain calculations relating to the amount of matter removed in solution by river water; but we have been led up by it to a consideration of the value of measurement and proportion in geology generally,

[1] *Chemical Denudation*, p. 49.

and the light that it helps to throw upon some complicated but nevertheless interesting problems.

Let us, as a further example, analyse the theory lately adopted by some scientific men, that the oceanic and continental areas are permanent and uninterchangeable. But first it is worth while pointing out that there is not one argument—at all events one that I can call to mind—which has been used in its favour that is not directly or indirectly based on negative evidence, the most unscientific of all evidence, from the difficulty, nay, often impossibility, of adequate proof. Thus it is said there are no deposits among the rocks equivalent to the deep sea oozes and red clays; there are no oceanic islands but what are volcanic; there are no indigenous land-mammals on oceanic islands. In this way is built up a tremendous theory which is to supersede all the older geological notions. It seems to be overlooked while framing these all-embracing propositions that what is known of the ocean has been obtained by the most superficial scraping of the bottom, at a very few points enormous distances apart, and a few soundings that bear an infinitesimal proportion to the extent of the ocean, yet which are supposed to give accurate indications of the form of the bottom.

Note, July 1903. *In the voyage of the 'Gauss' from the Elbe to Cape Town, soundings were taken with tubes of* 2 *to* 3 *centimetres diameter and* 200 *centimetres long, by which some long cores were obtained. One of these from the depth of* 7,230 *metres* (3,950 *fathoms*) *in* 0° 11′ *S.,* 18° 15′ *W., showed distinct stratification. The core was* 46 *centimetres long; the uppermost* 13 *centimetres consisted of red clay containing numerous fragments of volcanic rock, then followed in order four bands of different colour, passing from brownish-grey to dark and then light grey. The dark grey layer distinctly resembled a terrigenous deposit, and the light grey layer, the lowest of all, was the only one containing a perceptible proportion of calcium carbonate. A still more curious specimen was a core* 69 *centimetres (say* 2 *ft.) long, obtained in* 35° 52′ *S.,* 13° 8′ *E., from a depth of* 4,957 *metres* (2,750 *fathoms*). *The uppermost* 11 *centimetres consisted of a brown clayey quartz sand with very little volcanic or calcareous material, while the next* 12 *centimetres were almost pure Globigerina ooze, with fragments of the upper layers, and the greater mass of the section consisted of material similar to the upper layer, but with the clayey material predominating over the sand. Dr. Philippi could not account for this appearance of sand in a pelagic deposit by considerations of the prevailing wind, as it blows towards, not from, the South African deserts, nor by currents.—'Firstfruits of the German Antarctic Expedition,' Nature, July* 3, 1902, *p.* 224.

Let us now for a moment look at the problem from the point of view of the geologist who takes the trouble to measure and balance before pronouncing an opinion. Firstly, if we except granitic and igneous areas, there is no known place on any land on a coast line where stratified rocks of earlier or later date do not exist; or where, if they do appear on the surface, the oldest rocks are not met with when a bore-hole is put down sufficiently deep. In a word, the framework of continental land at the coast differs in no respects from the inland areas.

If land areas have been, as contended, permanent, the stratified rocks of which they are composed must have a beginning and ending somewhere—what engineers would call a 'limit of deviation' must have been marked out for them somewhere—where does this limit exist? Surely, at the present time, this limit should, at some point or other, be near the coast; if so, where is the evidence of such a thinning out and shading off? I hope I may make my meaning sufficiently plain; the stratified deposits—I speak of the older rocks, not the modern littoral deposits—must, if the theory be true, thin off from the continents somewhere. If at only one small place this could be pointed out, we should have some basis of fact to go upon. Dana says that the Laurentian rocks everywhere underlie the land areas. Considering that fluctuations of the limits of continents have to be allowed for on any theory, this shows that, as notwithstanding these oscillations we are not anywhere on the edge of the stratified rocks laid down untold millions of years ago, they must have a very extensive development under the sea bed. Let us pause to consider what is taking place now. The Atlantic Ocean, as I have shown, receives the waste of 21 millions of square miles of land forming its basin.

The greatest continental rivers—the Amazons, the La Plata, the Orinoco, the Congo—deliver their waters directly into the ocean. The Mississippi debouches into the Gulf of Mexico, the St. Lawrence deposits are largely intercepted by the great lakes; but by far the larger areas of the surface of the two Americas are so canted that either directly or intermediately the deposits are carried into the Atlantic Ocean. How long this has been going on I will not now stop to inquire, but in the course of time, unless some change takes place, the land will be worn down to the sea level, and the continents become

lagoons. Or if, on the other hand, the land were to keep on rising, the whole of the stratified rocks would be worn down to the fundamental rock of the globe—whatever that may be—an event that has never yet to our knowledge occurred in the geological history of the earth. Now, assuming for the sake of explaining my meaning that the river deposits are confined to a strip of land extending 200 miles from the coast (which I hesitate to admit), if the denudation of the 21 million square miles of land were to keep on its present lines the result would *be a shoaling of the present deep water and a pushing seaward of the littoral deposits.* This would not cease so long as the rivers continued to bring down any matter to deposit.

But if the world is to last on the old lines—if its geological history is to continue as hitherto—*a time must eventually come when these deposits will be uplifted and made into land. It is pretty well agreed among all geologists now that such an upheaval is an extremely slow process, and it is not improbable that the rivers would continue in their present courses, and cut through the deposits as they arose.*[1] This would push out the detritus still further, and lay it down on the flanks of the upland chain of mountains, if such there were. It is easy to see that in time the deposits would travel round the globe, for if we assume that eventually the continents get a cant in a direction opposite to what obtains now, the same process would be repeated on the opposite coasts. No amount of catch pits in the shape of inland seas that either Nature or human ingenuity could devise would suffice to stop this travel of material.

When we consider that the very oldest rocks everywhere underlie the land, that they reappear in islands which it is the fashion now to speak of on that account as non-oceanic, but which lie at considerable distances from the great land areas, and so sketch out in positive evidence their *least* extent at those early times; when we consider that the Island of South Georgia, separated by 1,200 miles of ocean from South America, is composed of clay slate, and has an Alpine character[2] and high mountains, it will seem incredible that the waste of the land could in any way be confined to the limited areas which the hypothesis of the permanence of

[1] Captain Dutton shows plainly that this has taken place on the West of the Rocky Mountains. *Tertiary History of the Grand Cañon District.*

[2] See 'Island of South Georgia' (*Geological Magazine*, Dec. 3, vol. i., No. 5, 1884).

oceans and continents demands. At all events, I think that those who hold the theory should have some consistent way of explaining its *modus operandi*, instead of confining themselves to vague generalities or making phrases do duty for ideas. What I should like to see explained is this: Where do the rocky formations we see on the land thin out and end seawards? and by what process during the fluctuations of the land have the deposits been kept from getting outside the areas the theory requires them to be laid down in? In fact, what sort of a basin is it over which a particle of detritus can never travel? But let us see if figures will aid our conception of the problem.

The area of land draining into the Atlantic, as I said before, has been estimated at 21 million square miles. If we assume the denudation of every kind over this surface to equal one foot in 3,000 years,[1] that will amount to one mile in 15,840,000 years, say 16 million years.

The area of the South Atlantic, from the equator to the 40th parallel, is, I have ascertained by careful measurement on the map, 10,239,600 square miles, say ten million square miles.

The area of the North Atlantic, from the equator to the 40th parallel, is in round numbers 11 million square miles; that is, the area of the Atlantic from the 40th parallel of latitude north to the 40th parallel south contains an area approximately equal to the area of the land draining into both the North and South Atlantic. I doubt very much whether the mean depth of this part of the ocean equals two miles; but, for the sake of calculation, let us say two miles. It follows that in 32 million years the detritus of 21 million square miles of land, at 1 foot in 3,000 years, would fill up and level the whole of this vast basin. But let us take a more restricted area. The river Amazons and the Congo, the two largest rivers in the world as regards water volume, deliver their floods into the Atlantic Ocean from opposite continents, and nearly at the equator. The combined area of their basins is probably not short of 4½ millions of square miles. The area of the Atlantic below the parallels 10° N. and 10° S. is 4,800,000 square miles. The whole of this vast cavity, taking into consideration the enormous volume of their waters, which means an increased rate of denudation, would probably be levelled up by these two

[1] I take these figures because they have been used by others, and are near enough for my purpose.

rivers alone in 20 million years. These two great rivers deliver their waters into the ocean direct; yet, in the face of this fact, we are told by some geologists that most of the deposits we are geologically acquainted with have been laid down in inland seas! Now I ask this simple question, By what possible arrangement of inlaid basins, gulfs, or lakes could this vast degradation of the land be kept within the limits necessary to avoid the land absorbing or taking the place of the ocean? If the rate of denudation were greater in earlier ages of the earth's history, as some assume, this only makes the *time* less and our difficulties greater! Now mark further, for this is of special importance. The Red Clay deposits which are responsible for much of the latter-day theorising, and which are supposed to prove that the ocean bottom has never been upheaved, are carefully mapped out in Mr. Murray's map. I have transferred them to the 'Geological Map of the World' by Jules Marcou, a map to which I have been much indebted in investigating these problems. You will observe that they approach within from 300 to 400 miles of Rio Janeiro, and the same from the Cape of Good Hope, while they come close up to the West India Islands at Porto Rico, and to the Leeward and Windward Islands, which connect the West Indies with South America, and which all admit were once joined to it. It follows, therefore, on the hypothesis of 'permanence,' that though the continent of South America was in existence, and extended at least as far eastward as at present, at an early geological age—as proved by the crystalline rocks of the Brazils—either the deposits, through all the geological time that has since elapsed, have been unable to travel out over a space of from 50 to 400 miles, so as to shallow the sea and cover up the deep-sea deposits, or what is now deep sea was formerly shallow sea, so that the Red Clay is precipitated on preceding deposits of another nature. The first alternative is too absurd for acceptance by any one who has accurate perceptions of geological time—or even any conceptions at all—and the latter disproves the supposition that the presence of Red Clays is in any way a proof of the *permanence* of oceanic areas. The existence of profound depths of the ocean within a comparatively short distance of the land is fatal to the supposition that the land areas have been in their present positions through all geological time. They point rather to vast changes of relative level having taken place in

the crust of the earth during geological time—under the ocean as well as on the land. The same argument will apply to the Cape of Good Hope, for there the oldest crystalline rocks also exist. It has also been proved by the soundings of the U.S. Fish Commission steamer *Albatross*, that in N. lat. 37° 12′ 20″ W., long. 69° 39′, or some 300 miles S.E. of Cape Cod, the bottom is no less than 2,949 fathoms, or over 3⅓ miles deep. The bottom in all soundings below 1,000 fathoms was found to be 'mainly composed of Globigerinæ ooze, usually having the consistency of fine sticky mud, commonly of a dull olive-green or bluish colour. When washed with a fine sieve the deposit is found to be mixed with a considerable amount of very fine siliceous sand, among which are some grains of magnetite and garnet.' It will be interesting also to those who speak so confidently of the depth of ancient oceans to know that a 'considerable number of our shallow-water species have been found to have a much greater range in depth than was anticipated, many of them going down below 500 fathoms, while some even go below 1,000 fathoms.'[1]

If we consider that the island of South Georgia, situated on the same parallel as Cape Horn, the southernmost part of South America, is separated from it by 1,200 miles of ocean[2] and is composed of clay slate, we cannot help seeing how absurd such ideas seem when put to the test of facts and figures. It has been thought by some that if it could be found that the island of South Georgia were once united to South America the difficulty of its existence would be got over; but it is plain that, so far from proving the hypothesis of the permanence of oceans and land areas, such a former connection points to the opposite conclusion.

I trust I have now shown what an intimate relation exists between a correct conception of the magnitudes of the various parts of the earth, the rate of denuding agencies and of geological time, and the great and interesting geological problems which we strive to understand.

This knowledge is only to be obtained by constantly reducing our otherwise vague ideas to figures, so that we can realise, handle, and weigh the various parts we attempt to reason about. This is the more necessary as the ordinary means adopted of conveying the information is the source of most

[1] A. E. Verrill, *Amer. Jour. of Science*, 1884, pp. 213–14.
[2] 'Island of South Georgia' (*Geo. Mag.*).

people's difficulties. Vertical dimensions are exaggerated, and maps of the globe necessarily distort the parts. Even those who know this are nevertheless affected by the conventional method of representation adopted in cartography, and the only way to throw off this influence and realise the value of figures is to work them out oneself. This may seem humble work, but geology has now arrived at a stage in which for its further advancement scientific precision is required.

CHAPTER XXI

THE NORTH ATLANTIC AS A GEOLOGICAL BASIN[1]

CONTINUING the line of investigation sketched out in my last address,[2] I propose to consider in what way the enormous amount of mineral matter annually poured into the Atlantic Ocean is distributed. Deposition is the first stage in the reconstruction and renovation of the earth's crust, as denudation is the last stage in its destruction.

It follows that if we can glean anything like accurate ideas of these two great and complementary processes of Nature, we shall have advanced a step towards a solution of many of the great and still unsolved problems which the geological history of our earth presents to curious and inquiring minds.

It is well to admit at once that the inquiry presents many difficulties, chiefly arising from the fact that the great bulk of the deposits are entirely out of view, and our only direct knowledge of their nature and distribution is limited to superficial dredgings in various depths of water. Still, the science of Geology has advanced sufficiently to enable us in many cases to say what amount of matter has been removed from a given area within a given geological time with some approximation to truth, while the rocks themselves bear witness to the conditions under which they have accumulated and the manner in which they have been built up. It is by piecing together more or less connected facts and mentally putting them in relation to each other that we may be enabled to penetrate some of the mysteries of Nature which cannot be attacked in a more direct manner.

Before entering upon the question of sedimentary distribution, it will be necessary to glance at what is known of

[1] Presidential Address to the Liverpool Geological Society (Session 1885–6).

[2] 'Denudation of the Two Americas' (*Proc. of L'pool Geo. Soc.*, 1884–5).

the form of the ocean bottom. It is only of late years that anything has been accurately ascertained respecting even its depth. There was a general idea prevalent that the depths of the great oceans much exceeded what is now known to be the truth. Still, the information is yet so extremely limited that we should pause before attempting generalisations from what little we know, and still more from surmises respecting those parts of which we have no accurate information.

Through the enormous traffic which now crosses the North Atlantic, the soundings taken in connection with the oceanic cables, the continental coast cables, the soundings and dredgings of the *Porcupine*, *Challenger*, and *Talisman*, the United States Coast Surveys and investigations of the Fish Commission, and the surveys of our own navy, more is known of the form of the North Atlantic basin than of any other oceanic area. By the kindness of Sir James Anderson, the eminent telegraph engineer, I am enabled to lay before you a map of the Atlantic between the equator and 60° N. latitude, with contours of the bottom approximately marked thereon, in stages of 500 fathoms.[1] No doubt much of this is inference, and we must bear that in mind while embodying our opinions.

It will be observed on studying this map that the portions enclosed by the 3,000-fathom contours are small in relation to the entire area of the oceanic waters represented. The 3,500-fathom contour encloses a very limited area situated in lat. 27° N., and long. 60° W., and another nearer to the Antilles. It is also noticeable that the contour lines approach one another as the continental coasts are neared. Not less remarkable is the fact that an area of depression below the 2,500-fathom contour exists in the Bay of Biscay some 60 miles from the N. coast of Spain and 200 from the S.W. coast of France.[2] The 2,000-

[1] Reproduced in *The Origin of Mountain Ranges*.

[2] The following is extracted from the *Engineer*, November 27, 1885, page 421 :—'Direct Spanish Telegraph Company's Cable, between Kennack Cove, Cornwall, and Bilbao, Spain, broke down on the 11th of October, 1885, and was repaired by the Eastern Telegraph Company's steamer *Electra* on the 9th of November. The repairs were effected in three working days, although the depth of water was 2,300 fathoms, more than $2\frac{1}{2}$ miles. Only three drags were made for the cable, it being caught and lifted in each case, but in the first haul the cable broke in the grapnel, the ship being a little too far from the broken end. Another cable, between Otranto and Corfu, laid twenty-four years ago, was raised from the bottom in 560 fathoms and repaired.'

fathom contour connects Madeira, the Canaries, and the Cape de Verde Islands with the African mainland, while on the opposite or E. coast of America no outlying islands exist. The 2,500-fathom contour runs close to the Antilles or Bahama Islands, and follows the American coast pretty closely as far north as Newfoundland, when it returns south again. The West India Islands, the Caribbean Sea, and the Gulf of Mexico are cut off from the deeper portions of the Atlantic by the 500-fathom contour, although there is a large area—55,000 square miles—in the Gulf of Mexico[1] below the 2,000-fathom contour. The Mediterranean is a repetition of the same conditions on the old continent, being a deep basin with only shallow communication with the Atlantic.

The Azores are situated upon a central area connecting the N. coast of South America with Iceland, and enclosed by the 2,000-fathom contour. It is of this area that we want much fuller information. It will be observed that on the route of the Anglo-American cable of 1880 there are marked a series of small depressions below the 2,000-fathom contour; while on the Jay Gould line there is an elevation with only 700 fathoms of water above it—lat. 50° N., long. 29° W. On the 'Anglo,' 1869, there is an elevation with 1,130 fathoms over it, in lat. 48° N., long. 29° W. In lat. 42° 30′ N., long. 29° W.—I give the positions only roughly from the chart—there is an elevation rising to within 18 fathoms of the surface. In lat. 28° N., long. 40° 20′ W., there is a sounding of 600 fathoms. Between the latter and the Azores, a distance of 1,000 miles, there are no soundings on the chart.

It would appear from these examples that there is a ridge about long. 29° W. running N. and S. for a considerable distance through 10 degrees of latitude, and it is extremely probable that it is connected with the sounding last given 1,000 miles to the S.W. of the Azores. It is also a noticeable fact that the closer the soundings are together the more striking are the inequalities of the bottom, which of itself should make us cautious in assuming that the bottom of the ocean is a level plain because a couple or so of soundings very far apart do not differ much in depth. The late Mr. Gwyn Jeffreys has stated:[2] 'While

[1] J. E. Hilgard: 'Basin of the Gulf of Mexico,' *Amer. Journ. of Science*, vol. xxi. p. 290 (1881).

[2] *Nature*, February 3, 1881, p. 325.

repairing, in 1878, the Anglo-American cable, a tract of rocky ground was discovered about 100 miles in length in the middle of the N. Atlantic, between 33° 50′ and 36° 30′ W. longitude, and about 51° 20′ N. latitude. Within a distance of 8 miles the shallowest sounding was 1,370 and the deepest 2,230 fathoms, a difference of 860 fathoms, or 5,160 feet; within 4 miles the difference was 3,180 feet, and within half a mile 1,380 feet. There are also the Laura Ethel Bank with a depth of only 36 fathoms, and the Milne Bank with 81 fathoms, both about 550 miles from Newfoundland, which is the nearest continental land. Other instances are the Josephine Bank with 82 fathoms, and Gettysburg with 30 fathoms; the distance of the former from Cape St. Vincent being 250 and the latter 130 miles, with intermediate depths of 1,700 to 2,500 fathoms.'

Nearer the coasts the inequalities of the bottom are very considerable. Sir James Anderson informs me that they 'find very great inequalities in the bottom from Lisbon towards the Canary Islands. Off the Burlings we found a crater nearly 1,000 fathoms deep, into which the cable ran, and we had afterwards to recover and re-lay it. On the top of the crater we found 80 fathoms soundings.' In reply to further queries Sir James says: 'As to the sudden increase of depth off the Burlings, I should call it a crater and not a depression, as it is only a few miles in diameter. All round it is under 100 fathoms, while the cavity is 1,000 fathoms.' 'As for valleys, they are abominably abundant and very precipitous.' 'For example: off the Burlings, lat. 39° 25′ 30″, long. 9° 54′ 00″, the ship had 1,300 fathoms under her bow, and sounding under the stern they got 800 fathoms. Off Lisbon and up the edge of the soundings there are great inequalities, which no doubt are a chain of mountains in the ocean. The problem we have to solve when the cable is laid over such places is by numerous soundings to trace out the valleys. Sometimes we succeed, but sometimes we do not, as often within half a mile there are great inequalities, and it would be impossible to sound the whole ocean every half-mile.'

Thus by the evidence of practical men, on whom devolves considerable responsibility for the success and maintenance of the great sea cables, we are further strengthened in the opinion that the ocean bottom is full of inequalities, contrary to the recent dicta of some scientific theorists who regard the ocean

bottom wholly as an extended plain sloping, and that not rapidly, at the borders towards the continental shore lines. The more it is found necessary to sound the ocean, the more apparent, I am convinced, will these inequalities become. A very good example of the truth of this remark has just come to hand, as Professor Verrill, describing the work of the U.S. steamer *Albatross* in 1884,[1] says that five stations in depths below 2,000 fathoms were between N. lat. 36° 05′ 30″ and 37° 48′ 30″, and between W. long. 68° 21′ and 71° 55′, while the chart shows a sounding immediately north of this area of 250 fathoms.

Having now glanced at some of the characteristic features of the North Atlantic Ocean bottom, it will be necessary to direct our attention to the rivers delivering into it, and in order to further extend our inquiry we may assume that the South Atlantic is only an extension of the same basin, and that its main features do not radically differ from those of the North Atlantic. To simplify the inquiry, we will confine our attention to the greater continental rivers, those I treated of in my last address—viz. the St. Lawrence, the Mississippi, the Amazons, and the La Plata on the American continents, and the Congo on the African continent, with perhaps a glance at others elsewhere as the exigency of illustration demands. One of the most striking facts connected with those great rivers, whose embouchures have been tested by borings, is the great depth of deposit that has to be penetrated before the original bed of the river is reached. In the case of the Mississippi it is not less than 630 feet below the sea level.[2] Unfortunately, I know of no borings in the St. Lawrence, the Amazons, or the other rivers named, but it is more than inferentially probable that their original beds are much depressed. The forms of the mouths of the La Plata and the St. Lawrence are better explained by subsidence than by any other hypothesis. But not only is the original bed of the Mississippi much depressed, but the grade of the river valley is so low now that to bring it back as a working river to its original condition it would appear to require a general elevation of a large part of North America. If we do not adopt this view, the alternative seems to be that an enormous thickness of rock has been worn off its basin at an extra-

[1] *Amer. Journ. of Science*, November 1884, p. 378.

[2] Professor Hilgard, Messrs. Humphrey and Abbott's Report.

ordinarily low inclination.[1] Taken in connection with earth movements as shown elsewhere, it would appear that a general, as well as differential, subsidence is the readiest and the most reasonable explanation of its present condition. The ancient beds of most rivers are proved in many cases by borings not to possess a regular grade, but to be in what we may call wave lines, which is doubtless the result of differential subsidence.[2]

A boring in the old alluvium of the Narbada at Súkakheri, far from its mouth, shows a depth of 491 feet of alluvial deposits, finishing in lateritic gravel without the bed rock being reached.[3] It is also a very general if not universal rule that the lower deposits in river beds are the coarser, consisting of gravel, shingle, or boulders. This is true of the Ganges as well as of the Mississippi, and it is a pretty good evidence of a superior grade in former times, which again means subsidence by earth movements. Dr. G. M. Dawson has lately been led to infer from the position of certain Laurentian boulders in Canada, 700 miles from and over 2,000 feet above their origin, that the east coast was formerly higher in relation to the interior.[4] It is very generally considered by American geologists that there have been earth movements about the Great Lakes which have diverted the course of their outflows into other channels; and General G. F. Warren is of opinion that the drainage of Lake Winnipeg was formerly along the Minnesota and Mississippi, and considers that the buried river channels met with in that district are due to alternate elevations and depressions.[5] Facts of a similar nature pointing to differential movements of the continental lands upwards or downwards, as the case may be, might be multiplied had I time, but I think I have given sufficient examples to indicate my meaning.

[1] Professor J. W. Spencer thinks there is proof of a former elevation of the whole Mississippi Valley of 3,000 feet. —*The Mississippi River during the Great River Age*, page 5.

[2] Professor J. W. Spencer points out that the pre-glacial channel of the Mississippi has now a less slope than the modern surface, 'being respectively about 0·45 and 0·70 of a foot per mile along a direct line.' – *The Mississippi River during the Great River Age*, p. 6, Reprint.

[3] *Manual of the Geology of India*, p. 384.

[4] *Geo. Sur. of Canada.*—Report of Progress, 1882 3–4, p. 149 c.

[5] 'Valley of the Minnesota and Mississippi Rivers,' Engineer Dept. U.S. Army.

The foregoing facts might perhaps be held to invalidate the view that the main river channels of continents are of great antiquity. Rightly considered, I hardly think they do. The enormous mass of matter which it can be proved has been worn out in many cases to form the valley is of itself a proof of antiquity. No doubt the levels undergo great changes, and the conditions of the basins also suffer a correlative change, but there appears a tendency to revert to the original lines of drainage. A great depression worn into a continent cannot be got rid of except by a breaking up of the strata; and even this, when the grade is steep, does not always obliterate the river course. Captain Dutton has graphically shown that the degraded matter, of from 6,000 to 10,000 feet of strata, occupying an area of 10,000 square miles, has all passed down the Colorado to the Pacific, and this in spite of frequent upthrows on its course from Tertiary times until now.[1]

The Amazons basin is largely filled with Tertiary riverine deposits, which it is now clearing out and carrying to the sea. There are Tertiary deposits in the basin of the Rhine.[2] In every large river there exist undoubted evidences of antiquity, but to trace out the history of a great river system is a very perplexing problem in physical geology. Still, the fact of the existence of a great river system is an *a priori* proof of geological antiquity. There are certain permanent features of the earth's surface which no amount of subsidence or elevation seems to affect. Mountain ranges once formed remain mountain ranges until effaced by denudation or covered up by deposit. The mountains of North Wales, formed in Silurian times, remain mountains still. The Pennine chain of Carboniferous age is not yet effaced, though part of a range of similar age is known to extend under London and link the Belgian coal-fields with the Mendips. In many cases, where rivers are supposed to have preserved their course notwithstanding the elevation of mountain chain across their channels, I am inclined to take another view, and attribute it to the wearing down and backwards of the river channel, which initially flowed at a higher level than the ridge it crosses. Mountain chains must be considered to be permanent reliefs in the continental maps, and river channels less permanent, but still

[1] *Geology of the High Plateaus of Utah*, pp. 15–20.

[2] Belt: 'The Loess of the Rhine and Danube' (*Quarterly Journal of Science*, p. 17 of Reprint, January, 1877).

lasting marks of Nature's graving tools. It is evident, on a little consideration, that mountains and hills must have a great effect in fixing the direction of a river channel, and once it is deeply cut into a continent differential subsidences will not readily obliterate it or materially change its direction. If we admit, what I think can hardly be disputed, that great rivers are of enormous antiquity, we may well ask ourselves what has become of the material ceaselessly washed down into the sea with almost the regularity of time itself? We see that the valley of the Amazons was a valley in Tertiary times; and although there have been fluctuations of level in the continent of South America, as shown by Darwin, the Andes must have formed the 'great divide' between the Pacific and the Atlantic ever since the initial stage of elevation. It follows, therefore, that whether the Amazons, the La Plata, and the Orinoco have been absolutely permanent drainage lines or not, the material of the continent, whatever its form, must have flowed into the Atlantic Ocean. The Andes, according to Darwin, were founded, if I may use the term, in Cretaceous times; it follows that since then the ruins of these mountains must have gone partly to form deposits on the continent of South America, but mainly into the Atlantic Ocean.

An examination of the Atlantic chart shows what appears to be a prolongation of the American continents into the ocean in a bench of varying width following the coast line, at the edge of which a more rapid depression takes place. It is usual to look upon these benches as wholly composed of the solid rocks of the continent. It is quite as legitimate to assume that in many cases they are largely composed of sedimentary matter degraded from the continental lands. The soundings at the mouths of the Amazons for a distance of 60 miles rarely exceed 10 fathoms, and are in some cases not more than 5 fathoms, and for as far as 150 miles from the coast do not exceed 45 fathoms. It is certainly probable that this is due to a levelling up of the ocean bed, which, whatever its depth may have been, is only a question of time. At all events, it would be difficult to find a belt of land with such slight inequalities as are exhibited by the submarine plateau extending in the front and to the north and south of the Amazons' mouths. We know there have been strata of many miles in the thickness laid down in former geological periods without apparent break or unconformity. Nature is uniform in her action, and there is no reason that I

know of to assume that these operations have ceased. If not, then where are thick deposits more likely to be taking place than on coasts near the mouths of rivers draining areas that have to be measured by millions of square miles? Whatever the form of the bottom or depth of water into which the river delivers its burden, it will in time shallow it. The sea thus acts as a leveller—spreading out the deposits to a uniform depth—cutting off here and laying down there. These continental benches are, in my view, the true submarine plains—plains of denudation as well as deposit.

The soundings off the South American coast in the neighbourhood of the Amazons show a pretty uniform distribution of 'grey sand with black specks,' occasionally mixed with broken shells. There are also some banks of mud and sand and hard mud; but, according to the chart of the entrances to the Amazons I exhibit, grey sand mostly prevails. There do not appear to be any rocks—in fact, it is just the sort of deposit, judging by its extension and uniformity, we should expect to be of great thickness. This is, of course, and can be, only an inference; but if we believe in the antiquity of continents, if it be but from the Tertiary period downwards, the inference is a legitimate one. There is, of course, the possibility to be taken into account of the former extension seawards of the continent, such as would be caused by the fluctuating depressions and elevations referred to at the commencement of this address. Such fluctuations would have the effect of spreading the deposits over a larger area, but probably would not affect their distribution to the extent that we might at first suppose. I am sensible that in opposition to this 'levelling up' theory, the phenomenon of the 'Swatch of no ground' opposite the mouth of the Ganges may be pointed to; but, as far as I know, it is a special submarine feature that does not appear in connection with any other great river. It would be, therefore, unfair to argue from an exceptional physical feature, for such exceptions meet us everywhere in geology. It may be due to bottom currents, or to recent geological subsidence.[1]

The New Orleans boring penetrated deposits to a depth of 630 feet below the surface. Professor Hilgard is of opinion that

[1] Mr. Fergusson is of opinion 'that the sediment is carried away from the spot, and deposition prevented, by the strong currents engendered by a meeting of the tides from the east and west coasts of the Bay of Bengal.'—*Manual of the Geology of India*, p. 408.

the river silt does not exceed 108 feet, and the remainder of the deposits are marine, not post-pliocene. It seems to me pretty evident that the Mississippi contributed the materials of which the deposits were made up; and it is a matter of little moment whether they were laid down as an alluvial delta or as a submarine extension of it. The controversy with Sir Charles Lyell as to the actual age of the delta seems to me to have been carried on without its bearings being clearly understood. The circumstance of the deposits of a river being freshwater, estuarine, or marine depends on the conditions of its embouchure. They may be any of these three. If the river delivers into deep water the deposits will be mostly marine until they level up the sea-bottom, when they may change to estuarine, and again to river silt. The time occupied by these changes will be determined by several factors: the amount of sediment brought down annually, the depth of the sea into which they are delivered, the strength, set, and permanency of the sea- or ocean currents, and the rate, range, and direction of the vertical movements that seem constantly taking place in the land and sea-bottom, or the length of the pauses that have taken place in these movements.

In illustration of this, I may mention the boring at Calcutta, in the Ganges delta. Full details are given in the 'Manual of the Geology of India,' pp. 397–400. The bore was carried down to 460 feet below mean sea-level, ending in a fine sand intermingled with shingle, without the base of the delta being reached. The whole of these deposits, excepting possibly the last, were considered by Dr. Blanford to be of freshwater origin. At Umballa, between the Jumna and the Sutlej, a bore was put down in the Indo-Gangetic plain 701 feet, ending about 200 feet above the sea-level, without penetrating to the base of the alluvial deposits. It is therefore evident that the Ganges Valley, as far as penetrated, was levelled up by the alluvial deposits of the great river, at least as rapidly as the subsidence took place. It would appear from the New Orleans bore-hole that the subsidence of the valley of the Mississippi was too rapid in its first stages to be overtaken by the deposits the river brought down. Seeing that in the Jumna and the Ganges the bed rock was not reached at such great depths at points inland of the shore lines, it is a fair and probable inference that outside and seawards the deposits are in most cases enormously thicker.

The lower deposits penetrated at New Orleans were considered by some to be pliocene, and even miocene. I agree with Professor Hilgard that they are post-pliocene, but the fact that this difference of opinion should occur is a very good proof of *age*; and if, as we may legitimately infer, the Mississippi laid down the materials of which the deposits are composed, or we even account for them in any way by continental denudation, we must be prepared to admit also that the deposits which overlie the rocky floor of the sea at their mouths, and which may be considered as geologically modern, are of very great thickness.

In investigating the nature and extension of oceanic sedimentation, we must not neglect the possible existence of submarine currents. Sir James Anderson says in a letter to me: 'Perhaps the most marked experience we have had of currents at great depths was in the case of the Falmouth cable, near Gibraltar. At 500 fathoms the wire was ground like the edge of a razor, and we had to abandon it and lay a new cable well in shore. Captain Nares, of the surveying ship *Nemesis* I think, with tangles could get no specimen of the bottom whatever, and he thinks he got sufficient evidence to prove the existence of a perfect swirl at that depth.' Again we have it on the evidence of Professor Verrill that the deposits under the Gulf Stream, even at great depths, do not correspond with the *Challenger* experiences. Large blocks of sandy clay were brought up from 1,060 fathoms, and large masses of hard but sticky greenish-blue clay from 1,168 fathoms, and between 2,000 and 3,000 fathoms in all the ten localities 'Globigerina Ooze' occurred. The Red Clay which, according to the *Challenger* investigations, distinguishes such depths was not once met with.[1]

According to the late M. Milne Edwards, the *Talisman* soundings showed the bottom of the Sargassum Sea to be formed of 'a thick layer of a very fine mud of a pumice nature covering fragments of pumice and volcanic rocks.' Between the Azores and France the sea bed 'is carpeted throughout this region with a thick white mud composed almost exclusively of Globigerinæ, and covering pumice deposits and fragments of various kinds of rocks. Some of these rocks brought up in our nets bore the impress of fossils, amongst

[1] 'Marine Fauna and Deep-sea Deposits off the Southern Coast of New England' (*Amer. Journ. of Science*, November, 1884).

others of Trilobites. But what more surprised us was to find, at a distance of over 700 miles from the European coast, pebbles polished and striated by the action of ice.' The sea bed stretching westward of Morocco and the Sahara was found to be 'extremely uniform, no longer presenting those rugged reliefs that had so impeded our operations on the coast of Spain.'[1] Professor Sars says the extensive depression between Norway, the Faroe Islands, and Iceland appears everywhere below 1,000 fathoms 'to consist of a very peculiar loose, but very adhesive, exceedingly light, nearly greyish white clay, which is strongly calcareous, and on being washed or passed through a sieve appears to consist almost exclusively of shells of a little low organism belonging to the Foraminifera Biloculina.' 'The Biloculina clay of the cold area contains a greater quantity of lime than the Globigerina clay of the Atlantic.[2] At a distance of from 70 to 140 English miles from the coast of Norway, at the edge of the submarine barrier, in depths of from 300 to 100 fathoms, the bottom is generally hard and stony.' 'Numerous rolled stones, whose smooth rounded forms and worn edges clearly enough show that they had at one time been subjected to the powerful action of ice, lie here strewn on the sometimes very uneven bottom, consisting of solid rock, and prevent the dredge from acting properly.'[3] This submarine platform as a rule rises somewhat towards its outer edge before it slopes towards the great depths lying beyond it, and simultaneously assumes, as in the well-known case of the Storegg, a hard stony character. 'At the first sounding, when we went out from Husoe, we struck this edge at about 140 English miles distance from the coast. The bottom, which before had everywhere appeared to be soft, suddenly at a depth of 221 fathoms became hard and stony, and retained this character even after it had sloped about 50 fathoms down towards the deep sea lying beyond.'[1]

The difference in the nature of the submarine plateau off the coast of Norway and the belt of shallow soundings off the mouth of the Amazons—which shallow belts more or less

[1] The *Talisman* Expedition.—*Nature*, December 27, 1883.

[2] Mr. Gwyn Jeffreys says the *Porcupine* dredgings gave from 50 to 60 per cent. of carbonate of lime. -*Nature*, Febuary 3, 1881, p. 326.

[3] The Norwegian North Sea Expedition of 1876.—*Nature*, March 8, 1877, p. 412, vol. xv.

[1] *Ibid.—Nature*, vol. xv. p. 436.

distinguish several of the coast lines of continents, especially near the mouths of great rivers—appears to be that the former is a submarine extension of the continental framework of the land, while the latter is the effect of accumulated sediment. The deeply serrated coast line of Norway indicates a considerable subsidence, for these great fjords, profound in depth, running up miles into the heart of the land, have undoubtedly been formed by the denudation of subaërial agents at a level much above the sea line. Under these circumstances we find the rugged nature of the country continuing far seawards under the rolling waves of the Atlantic. In the case of great continental rivers discharging into the open ocean or deep seas, the soundings are shallower and comparatively regular up to the edge of the great oceanic slope.

In conclusion, I am very well convinced from the reasons already given, namely, the great age and permanency of the river basins—such as the Amazons, which has certainly existed since Tertiary times—the depth of alluvial and other modern deposits at the mouths of most great rivers, the existence of extensive platforms about their mouths with uniform shallow water over them, the absence of rocks and presence of modern sediments, that deposits are now taking place in the sea thousands of feet thick, and parallel to those which we know have occurred over and over again in geological history. These deposits are, in my view, submarine extensions of the true deltas, which will make themselves felt in future geological history.

These considerations open up many geological questions of surpassing interest, of which we have hardly yet touched the fringe. The further we penetrate the unexplored regions of physical geology the larger appears the extent of country before us. Deeply sensible as I am of the slight and imperfect character of the sketch presented to you to-night, I trust that it may not be entirely barren, but may be found to contain some ideas that will germinate and yield further fruit.

CHAPTER XXII

TIME AS A GEOLOGICAL FACTOR

THE importance of time as a factor in geology has, in relation to denudation, been more or less understood and admitted by geologists since the days of Hutton and Playfair. Not so, however, is it realised that time plays an equally important part in geological dynamics.

It is well known to those who have to deal with practical mechanics that *time* is an element which has to be considered and contended with. A beam carrying a static load may be perfectly safe when newly subjected to it, but let the load remain long enough and a permanent set is given to the beam, ending in collapse. In the same way chains, ropes, and similar materials subject to tensile stresses weaken independently of actual wear or attrition.

The molecular changes that take place in all materials under constant stresses are not well understood, but it is within the practical experience of most of us that a bar too rigid to bend on the application of a comparatively sudden force will be pliable to one gradually applied.

In the same way rocks act in the heart of the

earth's crust, and even the most rigid are moulded by the complex stresses they have to contend with. Subjected as they are to enormous vertical pressure, increasing with the depth, when lateral pressure is developed, the various beds are moulded into forms frequently having considerable symmetry. On the other hand, fractures often take place due to the differences of rigidity of the several associated beds.

Again, with normal faulting a new set of stresses are introduced—namely, tension and those due to vertical shearing and compression—which have already been discussed under the head of 'Contraction Faults.'

The proof that all these forces have been applied gradually is to be found in the beds of rigid rock often symmetrically bent as if they were sheets of a ductile metal. Every material is plastic to competent pressure applied sufficiently slowly.

The catastrophic elevation of a mountain range is an event that could only happen were it possible that energy could accumulate in the earth's crust sufficient to bring about suddenly such a vast upheaval. The sudden upheaval of even a small mountain chain would shake the earth to its foundations, while that of, say, the Alps or Alleghanies would go far to destroy the whole habitable globe. But this event is in my philosophy an impossibility. Long before such a vast store of energy could accumulate, the earth's crust

would adapt itself by recurrent small movements to the forces acting upon it. Everything in geology leads us to think that mountain ranges are built up by gradual and successive creeps, and that a sudden release of pent-up forces takes place on a scale not larger than what is experienced in a great earthquake.

The symmetry of the folds seen in the Appalachians and other mountains of that type are eloquent proofs of the extremely gradual way in which they have been built up.

That these forms of upheaval are mainly due to lateral pressure is admitted by nearly all geologists. What, then, does this great fact mean? It means that for a sudden upheaval to take place sections of the earth's crust of areas extending to thousands of square miles must simultaneously move in a horizontal direction one way or the other over distances to be measured by miles—distances, be it observed, sufficient by the compression of the strata to raise up parallel ridges having the cubic contents of these great ranges.

Let us try to follow out mentally the consequences of such an extraordinary event as the sudden creation of a mountain range.

The movement of such an enormous weight over the earth's surface, were such a thing possible, would doubtless be accompanied by earth waves of appalling violence, shaking the whole lithosphere of the globe. Not only would this occur, but the redistribution of weight on the earth's crust and

the piling up of materials in the range itself would profoundly influence the structure of the earth's crust. The weight of the piled-up materials would press the crust down into the subcrust. It is almost needless to tell a geologist of the present day that there is no record to be found in mountain ranges of any such vast and sudden movements having taken place. Instead of a tumbled mass of materials, such as we may conceive would result from a sudden release of dormant forces competent to carry out such changes, we find order and symmetry and a perfect refitting together of the beds, to which operations we may suitably apply the term 'building' or 'mountain-building.'

The fact also must not be lost sight of that in many cases the material removed from the range by denudation is in excess of what remains, so that properly to appreciate the vastness of the operations involved in the sudden creation of a mountain range we must at least double the load that has to be acted upon and piled up in a restricted area.

And here it may be well to note that in the early days of geology, when catastrophic views were not unnaturally in the ascendant, it was supposed that mountains were created by simple upthrows or 'upheavals.'

Exactly what 'upheaval' meant it is difficult to find out. Probably it was one of those words that help to soothe the inquiring mind, even if they do not satisfy it.

We now know that, though uprise of compressed materials of the crust underlying the stratified lithosphere does take place, the principal forces have acted horizontally, and redistributed the load on mother earth by folding, compression, and by over-faulting.

This illustration is given as an extreme case of the application of catastrophic principles—or want of principles—to mountain building properly so called. Doubtless those who now consider that such events have constituted a considerable part of the geological history of our globe will divide each 'upheaval' into periods, and instead of making one event of it will make many. It is evident to any one possessing elementary mechanical knowledge that such periods must be limited by the stresses the lithosphere is competent to bear without moving; when this is exceeded the change takes place from the sudden relief afforded.

Therefore, looked at in its proper light, the difference in the views of the modern and the earliest geologists are those of degree only. Catastrophes may happen, but not on the stupendous scale invoked by the pioneer reasoners on geology, who, having few facts to go upon, imagined the rest.

The progress of geology is due to the careful way in which Nature has been examined and questioned, and to the application of principles established by kindred and maturer sciences to explain phenomena now more accurately recorded.

The tendency of this closer study of the records

entombed in the earth's crust is to show that in the domain of geological evolution all has been systematic and orderly. It becomes more and more apparent that the features of the earth's surface are the progressive result of the interaction of internal and external forces. If there be one thing more than another that impresses itself on the mind, it is the slowness with which these vast changes come about and the enormous length of geological time.

In the early stages of investigation the relations of one set of phenomena to those of another were naturally not apparent.

For instance, it was not known that mountain-building is always preceded by great sedimentation. This was a generalisation that could not be made until a vast amount of geological mapping had been done.

Furthermore, these sediments—the comminuted *débris* of earlier lands—cannot have been laid down otherwise than very slowly. Even if we assume that the area of deposit is but one-tenth that of the area of denudation, the accumulation of a thickness of deposit comparable with that exhibited in a large mountain range must have taken millions of years. But it is proved that there are rocks existing in which the accumulation proceeded much more slowly than the ordinary denudation of the land. In a most interesting paper by Professors Edgeworth David and E. F. Pitman it has been shown that there exist in New South Wales

Palæozoic Radiolarian rocks reaching a thickness of 9,000 feet, inclusive of submarine tufts, and composed chiefly of tests of radiolaria, which are present in the bulk of these rocks in the proportion of one million to the cubic inch. The rocks are estimated to extend a distance of 285 miles north and south. Dr. Hinde in a separate paper on the same subject considers that the rocks are of Devonian age, and he observes: 'For the formation of so great a thickness of rock composed principally of extremely fine calcareous or clayey materials filled with the remains of these microscopic organisms, an enormous period of quiet sedimentation must be conceded.'[1]

Thus we see that the regenerative forces of the earth wait upon the crumbling and destruction of the old lands. The one is consequent upon the other. A vivid conception of the slow pace at which these operations proceed is presented to us when we consider the extreme comminution to which the sedimentary particles composing the rocky envelope have been reduced. The chapter on slaty-cleavage emphasises this, as it does the slowness with which the dynamic forces brought into play by internal heat and pressure act in imposing new structures upon the rocks, and, figuratively speaking, provide the marble from which Nature's chisel carves out those varied features which make the beauty of the landscape.

[1] *Q. J. G. S.*, pp. 16 63.

The value of *Time* as a geological factor in the regeneration and reconstruction of the face of the earth, though it may not at first be apparent, assumes overwhelming importance with the increase of our knowledge of the mysterious yet systematic and purposeful ways of Nature.

CHAPTER XXIII

BEARING OF THE INVESTIGATIONS ON THE SUPPOSED PERMANENCE OF OCEANS AND CONTINENTS

IT will be of interest to consider in what way the physical and experimental investigations recorded in the preceding chapters affect the question of the permanence of the great oceans and the continents.

But it will be first necessary to ascertain what meaning is attached by the leading geologists who have written upon the subject to the term 'permanence.'

A study of the various views that have from time to time been put forward leaves an impression upon the mind that the controversialists have not all been discussing the same thing.

Dana seems to have originated the idea that continents have been built up about certain nuclei—portions of the crust of the earth that have first hardened--and that they have developed by a process of inorganic growth to their present form. Inferentially this would constitute permanency of the great land masses and the great oceans, but it was not formally stated in that way. Darwin favoured the view that the land masses have been

substantially in their present position since Cambrian times, and very properly condemned the readiness of some naturalists to build land-bridges across the oceans to account for every little difficulty that beset them in the way of geographical distribution of plants and animals.

The naturalist who gave the most definite form and fixity to these speculations on the permanence of the great land areas is undoubtedly Wallace, though in his later speculations he appears willing to admit that a larger area of the globe has been subject to fluctuating continental movements than he at first supposed.

As regards the great depths of the ocean he is still firmly of opinion that they have never been land during the geological periods we are acquainted with. Formerly, for practical purposes, he took the 1,000-fathom line as generally and roughly indicating the separation between the oceanic and continental areas. Further information has led him to consider that the 1,500- or perhaps in a few cases the 2,000-fathom line marks out the deeper and unchangeable portions of the oceanic basins.[1]

Intermediate between the extreme views held by Wallace on the permanence of continental and oceanic conditions and those of Lyell and the older geologists who considered that every part of the present oceans had once been land and every part

[1] 'The Permanence of the Great Oceanic Basins' (*Natural Science*, vol. i. p. 419, 1892).

of the land had once been sea, almost every combination of permanence and permutation has had its advocates.

There is, however, a general disposition among geologists to concede great geological antiquity to some of the deeper portions of the oceans, though such an eminent authority as Suess sees 'no reason why parts of the ocean or even of the dry land may not to-morrow sink to form new depths, or why we should believe that all the great ocean basins have been continuously covered by water since panthalassic times. So far as the Atlantic is concerned, there even exists some evidence to the contrary.[1]

It is not my object to discuss the whole aspect of the question, or to inquire minutely into the arguments made use of by the several eminent geologists in support of their views. That their opinions should differ one from another is not at all surprising when, as pointed out by Suess, they start from fundamental discrepancies of principle.

In reasoning on the subject, the first great difficulty met with is the absence of full and accurate information as to the submerged portions of the earth's surface. We know that the land

[1] *Natural Science*, vol. ii. p. 187, 1893. See also Dr. Blanford's Presidential Address to the Geological Society of London, 1890, and the very comprehensive opening Address to the Royal Physical Society of Edinburgh, vol. xiii., 1894 and 1895, in which the late Professor H. Alleyne Nicholson discusses the whole question. One of the earliest contributions to the subject was by myself, 'Oceans and Continents' (*Geo. Mag.* 1880, p. 385).

areas have been at various times submerged, because we find in them great thicknesses of strata full of marine fossils, in some cases reaching miles in depth.

We also know that the alternating conditions of dry and submerged land have occurred again and again on the same areas at various periods of geological time.

When, however, we begin to speculate upon the submarine conditions of the earth's surface, the data become of a more inferential character. If it were possible, as doubtless it may be at some time, to put down bore-holes in the bed of the ocean, the work of the geologist would become much easier and clearer. For instance, if even the deeps only have been permanent throughout geological time, a testing bore would reveal the geological history of the locality by an unbroken sequence of sedimentary deposits. It makes the mouth water to think of it! No unconformity, no missing link, no imperfection of the geological record!

As I feel quite certain it will never be my good fortune to behold such an interesting core, I must confine myself to pointing out what evidences of a physical nature do exist from which we may reason from the known to the unknown.

It has already been proved that, in addition to the geological records of the transgression of former seas and oceans over the sites of existing continents, numerous pulsations of upheaval and

subsidence, marked by marine terraces, have occurred in quite recent geological times.

On the other hand, recent risings and sinkings of sea-beds have been proved on equally good evidence.

The submerged or 'drowned' valleys found on almost every continental and island coast line speak eloquently of former elevation of the land. These sub-aërial features, together with the submerged escarpments traced by Spencer and others in the Antilles and on the American coast, reinforce the testimony of the submerged river courses.

But we have not to depend altogether upon soundings and Admiralty charts to know that these features exist. When borings have been made at the mouths of great rivers, no matter in which continent they are situated, they reveal the striking fact that the river-beds are considerably below the sea level. They are prolongations of their bed-rock valleys filled up with detritus, which usually increases in coarseness as the ancient bed of the river is approached. Precisely to what distance they may be continued under the sea-bed we know not. But it is here that the investigations of Spencer come in to help us, and the Admiralty charts offer their quota of evidence.

In numerous cases, like that grand exemplar the Congo, the valley may be traced by soundings far out seawards.

This universal marginal evidence of the sub-

mergence of sub-aërial features is a striking and, it long appeared to me, an inexplicable phenomenon. To account for it by a supposititious contemporaneous sinking of the whole of the dry land or continental blocks seems far-fetched.

My matured opinion, founded largely upon the investigations undertaken for this work, is that the present contemporaneous submergence of all these valleys can be satisfactorily accounted for only by a rise of portions of the floor of the ocean basins. The bulk of the oceanic waters has been, we may reasonably assume, a constant quantity within the limited geological period now under consideration.

If the theory of geomorphic changes set forth in Chapter I. be a true conception of the operations of Nature, there must have been during the long vista of geological time many changes of quiet upheaval and subsidence of the ocean floor. Such movements may, in the greater number of cases, have been concealed by the hydrosphere. The area of the great oceans is much in excess of that of the dry land, and the profounder depths may have been greatly affected without the bottom being lifted up sufficiently to form dry land. The effect of this shallowing of oceanic deeps would make itself felt by a rise of the waters on the bordering land, the universal submergence of the coastal regions, and the phenomena of drowned valleys.

On the other hand, the sinking of a portion

of the oceanic basins would be registered by horizontal marine terraces and deposits.

In all these speculations we must never lose sight of the fact that the hydrosphere is a mere film on the surface of the spheroid. An average of about $2\frac{1}{4}$ miles is the latest computation of the depth of the ocean.

This represents only about one seventeen-hundredth part of the radius of the earth.

A very small fluctuation of volume of a portion of the solid globe, such as might readily take place on the principles enunciated in Chapter I., would make a considerable difference in the distribution of land and water.

We must be ever on our guard against the natural tendency of the human mind to exaggerate heights as compared with areas.[1]

A sinking or a rising of the ocean-bed, and the consequent rising or falling of the sea level in relation to the land, although it would be universally felt, would be limited as regards vertical range. It would not account for the greater changes of level which occur both locally and over large areas, as detailed in Chapter I., nor for the warping of ancient shore lines or other numerous cases of differential movement detailed in this work. It would be essentially slow in action, extending over lengthened periods of time.

The evidences seem irresistible that the changes of level of land relatively to the sea

[1] See diagram, Plate I., Book I.

surface are in most cases mainly actual uplifts or downthrows. These absolute vertical negative and positive movements are superimposed upon minor risings and sinkings of the sea level.

If the earth be endowed with a mobility resulting from differential changes of volume, as set forth in the Theory of Geomorphic Changes (Chapter I)., the resultant effect would coincide with the recent and geologic changes of level registered in the earth's history. There would be slow and steady rises and sinkings of the sea level of a universal character, and large but in comparison local upthrows and downthrows of the dry land. These movements of the lithosphere would not be confined to the dry land; they would also be going on under the hydrosphere, but out of our ken.

The universal pulsations of rising and falling in the sea level are secondary effects of the warping of the lithosphere.[1]

Upon all these areal changes of level are superimposed the changes brought about by lateral pressure, which I have sought to show is due to recurrent expansion and consequent lateral creeping of the surface rocks. These movements are initiated by a long course of sedimentation, which in some cases creates deposits many miles thick. It is proved that the sea-bed has in places sunk under the load of sediment thrown down upon it,

[1] 'Theory of Geomorphic Changes,' Book I.

but it is doubtful if the bending of the crust is wholly caused by the weight of the deposit.

Most probably it is the coincidence of a sinking area with a great inflow of sediment that creates the abnormal thickening and immense concentration of mechanical deposits which, we have seen, is the precedent condition for the production of a great mountain range.

In the views I have elaborated in these pages and in 'The Origin of Mountain Ranges' there is little to favour the idea that the continents are permanent.

Their development has necessarily been slow, but a lateral spreading of sediments is always taking place round their borders. Many transgressions of the seas across the continents have occurred from age to age, as graphically described by Suess; but, although sediment may be brought to and be deposited on the sites of former lands again and again, there is nothing existing in the shape of a catchment even at the 1,500-fathom contour to prevent sediment drifting out and settling in the sea-bed beyond during one of the many mutations of level to which the earth is subject.

We may justly ask if the land fluctuated in elevation to the extent of 9,000 feet, what became of the sediment when the land was at its maximum height above the sea level? Would it not have spread out and encroached upon the sea where now are the great deeps? It seems to me the admission that changes of level up to 10,000

feet have taken place gives away the idea of continental permanence and leaves only the 'vasty deeps' consigned to permanent repose.

Earth-structure phenomena hinge so together, the one set of movements are so correlated to others, that it is necessary to take a very wide sweep of vision to make even an attempt at explanation. For instance, there is the case of the Island of South Georgia, in the Antarctic Ocean,[1] which I pointed out so long ago as 1884 was found by the German Expedition to be formed of clay slate, and that the presence of such cleaved sedimentary rocks was a clear proof that the island is only a small remnant of a large area of land, and the soundings appear to bear out this view.[2]

Again, I have shown that the bottom of the Atlantic is not a plain as assumed by some, but a diversified surface like that of the dry land, and that a large portion of it has at some former geological age been carved out by sub-aërial agencies.

It is satisfactory to have the support of Suess in this view, though his theory of how the Atlantic originated differs from the explanation advocated in this work.

Furthermore, it has been strongly urged that, as no deep-sea deposits exist on the land areas, it follows that the deep sea has never been over them. This, as I have frequently pointed out, is

[1] 'The Island of South Georgia' (*Geo. Mag.*, 1884, pp. 225, 226).

[2] See Chap. XIX. Slaty-cleavage, which proves that slaty-cleavage can only be induced in strata buried by vast masses of other rocks, which in South Georgia must have been removed by denudation.

arguing from negative evidence and so impossible of proof.

When such vast areas of the earth were and still are geologically unexplored it was, to say the least, a very bold assumption. Now we know that this statement cannot be supported, for such deep-sea deposits have been discovered in the form of Radiolarian cherts.

Looking at the question from a mechanical and dynamical standpoint, there seems to be no reason why oscillations of level should be restricted to 9,000 or 10,000 feet, yet the strongest upholders of the theory of permanence agree that movements to that extent have occurred in the past.

Even if we assume that no greater vertical movements than these have affected the great land areas of the globe, the problem of how such vast areas have from time to time been uplifted or depressed remains for solution.

This has been attempted in Chapter I., and if there be truth in my theory of geomorphic changes there seems to be no valid reason why 10,000 feet should be the maximum variation of level of any portion of the earth's crust.

In considering the bearings of these vertical movements even if limited to an uprise of the sea-bed not greater than the figures quoted, we must not forget that when the continents or portions of them were at their maximum elevation, the surrounding seas would be shallowing by the deposit of detritus brought down by rivers.

These operations of Nature take an immense time, and there must exist in the bottom of the ocean enormous masses of sediment which, in the course of geological time, will become dry land.

That the continents on both sides of the Atlantic have been raised simultaneously *en bloc*, and that the continental slopes represent sub-aërial escarpments bounding these blocks, it is difficult to believe.

The displacements would be so huge, if simultaneous, as to forbid acceptance. Nevertheless, there is, as we have seen, good evidence of a universal character that the land masses are bounded by marginal features pointing to sub-aërial agencies as the developers of the forms met with.

The obscurity of some of the sub-oceanic features is not to be wondered at. There is a consensus of opinion that Pleistocene or Quaternary time has been much more lengthy than formerly supposed. During that period, if not for longer, most of the big rivers have been pouring their loads of detritus into the seas and oceans.

By fluctuations of the level of land and sea these deposits have been laid down now here now there, sheeting and levelling the bottom of the ocean with new strata.

The main drainage lines have been preserved by these fluctuations of level, and the 'notches' or channelling in the continental shelf have only been partially filled up. If we add to this observa-

tion the difficulty of getting soundings sufficiently close together to develop the sub-oceanic forms—or what may be called the sub-aqueous orography—we may form some idea of the difficulty of constructing reliable bathymetrical charts.

At present, from the information at hand, we can form but a sketchy idea of the land features at the commencement of Pleistocene times. The details are, however, gradually growing, though it will be reserved for others to benefit by them.

There now remain the oceanic features called 'deeps' to consider. These, as already pointed out by Dana, cannot be volcanic in origin. They occur in non-volcanic as well as in volcanic areas.

They are, in my view, produced by a *sagging* of the earth's crust, similar to that which originated the Mediterranean basin.[1] They are not necessarily permanent.

Take the 'Sigsbee' and 'Thoulet' deeps; which are within 100 miles of the continental shelf of the North American continent. Had they existed as 'deeps' in former geological ages, how is it they have not been filled up by the rain of sediment continued through all that lengthened time? They may not be very old and not necessarily more lasting than the deep basins on continental land such as the Black Sea and Lake Baikal.

[1] By an expedition sent out by the Vienna Academy of Sciences, the greatest depth of the Eastern Mediterranean found was 3,700 mètres, or over 2¼ miles, near a great depression which runs between Malta and Cerigo.—*Science Gossip*, May 1891, p. 114.

The conclusion is forced upon us that movements and interchanges of such magnitude have occurred in the distribution of the oceans and land masses during geologic time that it would be a misnomer to call them 'permanent.'[1]

Slow indeed have been these geographic permutations, and, as pointed out in Chapter III., there has probably existed connecting land between them in one place or another through long periods of geologic time.

To sum up in one sentence, the changes are essentially forms of development, the permanence is that of land connection.

[1] Dr. R. F. Scharff, in a paper entitled 'Some Remarks on the Atlantis Problem,' read before the Royal Irish Academy, November, 10, 1902 (*Proc.* vol. xxiv., sect. B, Part 3), brings forward evidence of a varied character to support his contention that there has been at no very remote period a land connection between Europe and America.

CHAPTER XXIV

PLEISTOCENE RAISED BEACHES AND SUBMERGED FORESTS

Description of Plate XXXVII.

I AM indebted to Mr. J. J. H. Teall, F.R.S., Director of the Geological Survey of the United Kingdom, for the photograph of the Raised Beach north of Ballantrae, in Ayrshire, of which Plate XXXVII. is a reproduction. Between Ballantrae and Girvan many examples of Raised Beaches are preserved at different levels, mostly cut in Boulder Clay. The one represented is at a level of 50 feet above O. D. The beach or terrace is covered with gravel and sand.

PLATE XXXVIII.

Description of Plate XXXVIII.

Plate XXXVIII. is a view of one of the raised beaches near Vadsö on the Varanger Fjord, from a photograph supplied by Mr. Edmund Dickson, F.G.S., and taken in 1896. The ancient sea cliff is a well-marked feature in the landscape and is worn in a hard quartzose sandstone, the 'Gaisa Grit.' The terrace or beach is formed of angular or subangular stones in a sandy or gravelly matrix, the stones being usually more rounded in the lower terraces than the higher. Resting upon the surface were many boulders and some erratics. Mr. Dickson says,[1] 'In few places in Norway are the raised beaches so well exhibited as on the northern side of the Varanger Fjord. Behind Vadsö the terraces range one above another, the highest terrace being, according to Mr. Strahan, about 296 feet above the sea-level, and succeeding each other at so short intervals that they appear to run one into another, and render it almost impossible to trace out an individual terrace for more than a very short distance. As a rule we found the higher terraces were much better defined than the lower.' The terrace illustrated is about 200 feet above sea-level.

For further details see paper by Mr. Strahan, *Q.J.G.S.* vol. liii. p. 147.

See references in this work to these terraces, p. 9.

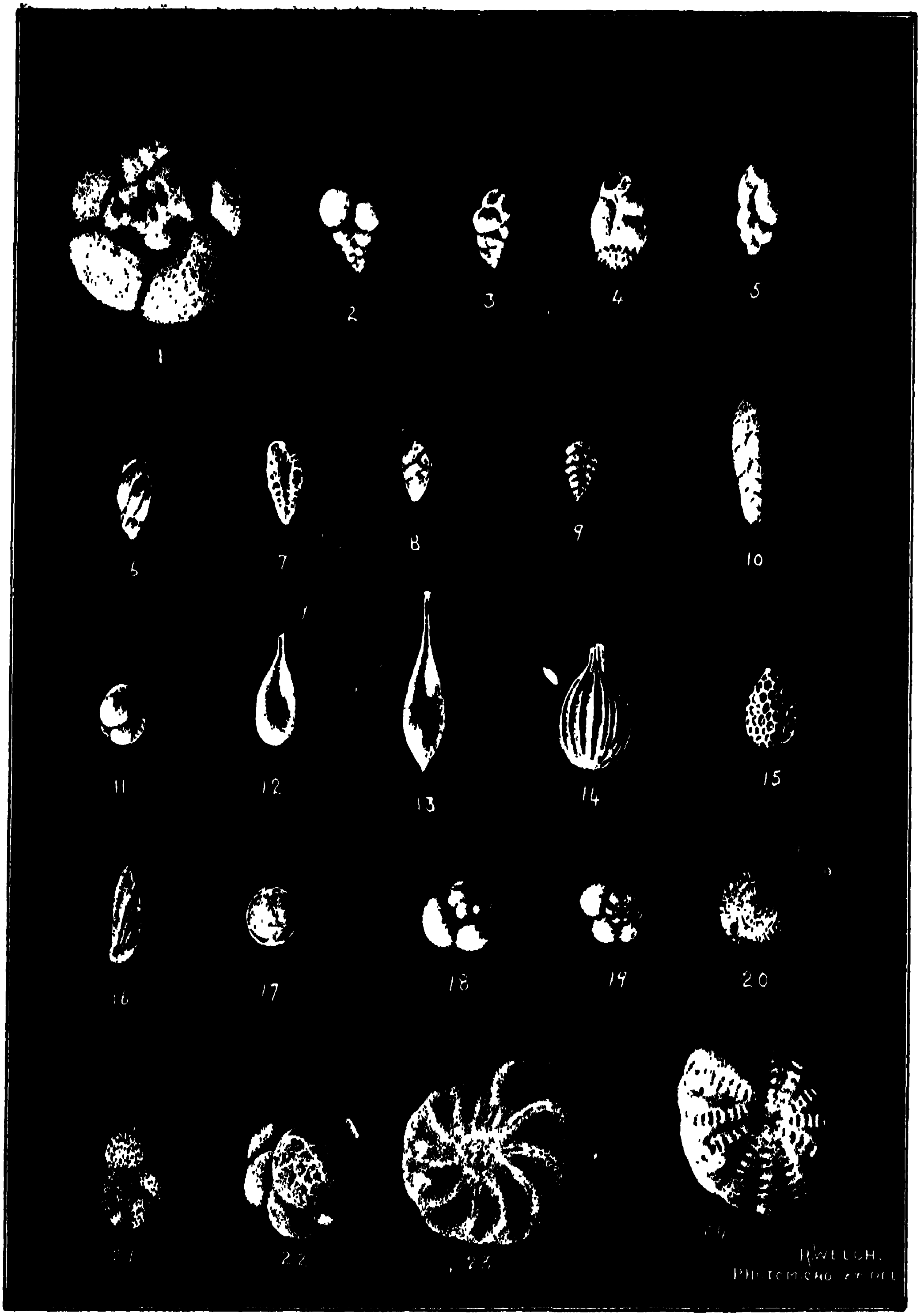
[1] 'Geology of the Northern Part of the Varanger Fjord,' *Proc. of L'pool Geo. Soc.* 1897, p. 135.

Description of Plate XXXIX.

The submerged forest represented in Plate XXXIX., enlarged from a photograph by Ward of Manchester, is a typical example of those surrounding the coasts of the British Isles. It is to be seen now on the shore at Leasowe in Cheshire, but has been much diminished of late by the extension of Hoylake eastwards and by denudation. There are the remains of a similar forest on the Lancashire coast at Blundell-sands, which, when I first saw it, was much more extensive than at present. No doubt the two forests were separated by the River Mersey, which was a fresh-water river at the time the trees grew.

PLATE XXXIX.

PLATE XL



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R. WELCH.
PHOTOMICRO ET DEL.

Description of Plate XL.

This Plate is from a photograph by Mr. Robert Welch of Belfast, illustrating a paper by myself and Mr. Joseph Wright in the *Geological Magazine*, entitled 'A Contribution to Post-Glacial Geology,' March 1900, p. 97, the block of which was kindly lent me by Dr. Henry Woodward. It represents the most characteristic of the Marine Foraminifera found in the blue clay underlying the Leasowe Forest bed.

Fig. 1. *Trochammina inflata* (Montag.).
" 2. *Textularia globulosa*, Ehr.
" 3. *Bulimina pupoides*, D'Orb.
" 4. " *marginata*, D'Orb.
" 5. " *fusiformis*, Will.
" 6. " *elegantissima*, D'Orb.
" 7. *Bolivina plicata*, D'Orb.
" 8. " *obsoleta*, Eley.
" 9. " " var. *serrata* (Chapman).
" 10. " *punctata*, D'Orb.
" 11. *Cassidulina crassa*, D'Orb.
" 12. *Lagena lævis* (Montag.).
" 13. " *clavata* (D'Orb).
" 14. " *sulcata* (W. and J.).
" 15. " *hexagona* (Will.).
" 16. *Cristellaria crepidula* (F. and M.).
" 17. *Lagena marginata* (W. and B.).
" 18. *Globigerina bulloides* (D'Orb).
" 19. " *cretacea*, D'Orb.
" 20. *Patellina corrugata*, Will.
" 21. *Discorbina globularis* (D'Orb).
" 22. *Rotalia Beccarii* (Linné).
" 23. *Nonionina depressula* (W. and J.).
" 24. *Polystomella striato-punctata* (F. and M.).

The figures are magnified 40 diameters.

References to the changes of level recorded by these Raised Beaches and submerged forests will be found in Book I.

See also 'Oscillations of the Level of the Land near Liverpool' (*Geo. Mag.* November 1896, p. 488).

CHAPTER XXV

CLOSING REMARKS

AS the knowledge of geology advances, the more clearly is it seen that the phenomena of our earth are related, and that there is little chance of their being explained unless we call other sciences to our aid.

Before I commenced the study of geology in the field, my practice as an architect and engineer had naturally brought me into contact with many physical problems, and it is owing to this education that I am enabled to approach their solution from a standpoint somewhat different from that of most geologists. It is to the aid afforded by the collaboration of scientific workers in their various branches *that we must now look for the advancement of one of the most interesting of studies*—the history of the evolution and development of the structure of the earth we inhabit.

That this study involves thinking of a high order is to be counted to it as a merit. Whether such speculations eventually are shown to be partially right or wholly wrong, they may possess great suggestiveness and lead to nearer approximations to the truth. But before theories can be established all the underlying scientific ideas must be brought to the test of practical experiment and quantitatively investigated. In my own case I have

aimed at this method of proof, and the result is embodied in this work.

To spin theories independently of the facts of Nature is a vain and unprofitable occupation, but it is now recognised by all scientific men that a working hypothesis is needful in scientific investigation. A bare collection of facts would prove as barren as too much hypothesis. But no collection of so-called facts, however bare, can be made without a basis of underlying assumptions.

The continuity of my work in the past is somewhat hidden by being distributed through so many Journals and Proceedings of scientific societies. Few, if any, would labour through these valuable tomes to get a connected idea of the principles I have been attempting to establish. In the 'Origin of Mountain Ranges' and 'Chemical Denudation in relation to Geological Time' certain of my views have been expounded in a systematic and connected manner, and it is the object of this work to complete and round off these 'Principles' and bring separate but connected investigations to a focus. Nearly the whole of the matter is original, and the greater part quite novel.

Doubtless I shall be asked why I did not include a chapter on Geomorphology and show how the present orography of the earth is largely due to denudation.

To this I reply in advance that the ground is already occupied by noted geologists, who have made themselves specialists in this study. Under

these circumstances I cannot do better than refer my readers to the work and papers of the late Beete Jukes, to W. M. Davis, Israel Russell, G. Karl Gilbert, and other English and American authors, not to omit the more popular expositions by J. E. Marr and Lord Avebury.

To have treated so wide and interesting a subject with justice would have required a large portion of a second volume, and this I was not prepared to undertake.

As a closing word I desire to apologise to those authors from whom I may have consciously or unconsciously imbibed ideas, for not specifically mentioning their names. It is very difficult to trace the origin of one's opinions. If the conceptions are good they soon get absorbed in general geological ideas and their ultimate origin is not easy to determine.

Finally, investigation proceeds at such a pace that knowledge is accumulated during the time a work is in the press which might add to the value of one's labours. To wait for such information would naturally cause never-ending delay, so I submit my little venture to my brother geologists, bespeaking from them what I feel sure will be granted, a fair consideration even for those views that do not meet with their approval.

With these few remarks I conclude my labours. Prosecuted in the intervals of professional work, they have necessarily been arduous, but at the same time they have afforded me much recreation and pleasure.

RECENT NOTES

1. *Experimental Mountain Building.*—After note 1, p. 195, was in type, Lord Avebury read an interesting paper to the London Geological Society (May 27, 1903, published in the 'Q. J. G. S.,' August 1903) describing 'An Experiment in Mountain Building.' The compression was effected in a square chamber 2 feet across and 9 inches deep. The materials acted upon were layers of carpet-baize alternating with layers of sand, the pressure applied being in two directions at right angles to each other. It is to be hoped, for the better understanding of the experiment, that Lord Avebury, in his promised further researches, will favour us with a diagram or photograph of his compression chamber.

2. *Rock-shattering by Differential Expansion.*—In a very suggestive paper on 'The Mechanics of Igneous Intrusion' ('American Journal of Science,' August 1903), Reginald A. Daly shows clearly how, by differential expansion, enormous stresses may be set up in a rock invaded by granitic or other igneous magmas. This, Mr. Daly thinks, accounts for the shattering of the invaded rock so often observed at plutonic contacts.

3. *Subsidence of an Antarctic Continent.*—Dr. Henry Woodward, in a very interesting Presidential Address to the Norfolk and Norwich Naturalists' Society (March 1903) entitled 'The Distribution of Life in Antarctic Lands,' reprinted in the 'Geological Magazine' (September 1903), says:

'It is probable that a very large extent of ancient land around the present Antarctic continent has been lost to us by submergence, and that the rather numerous small islands in the surrounding ocean are but the buoys or landmarks indicating large areas of more or less continuous land which has since disappeared. This is supported by the many signs of volcanic activity in recent times which these islands display. Doubtless land connections stretched from South America to the South

Shetlands, the South Orkneys, South Georgia, and to Kerguelen Island.' Also:

'That earth movements on a widely extended scale have *occurred in the South* is evidenced by the very late elevations *and subsidences which* have taken place in parts of the Andean *chain and in* Tierra del Fuego, also in Kerguelen Island, Eastern Australia, Tasmania, New Zealand, and the Chatham Islands.'

4. *Arctic Geology: Raised Beaches, Ellesmere Land.*—In a summary of the geological results of 'The Second Norwegian Polar Expedition in the *Fram*, 1898–1902,' Dr. Schei, the geologist of the expedition, says ('Geographical Journal,' July 1903, p. 64): 'The youngest evidences of marine action are the sands and clays, with sub-fossil remains of existing marine organisms which were observed at an altitude of 650 feet all round the coasts of Ellesmere Land. The altitude to which that sea rose is indicated for the most part by the loose materials of the marine terraces, although traces of it are not wanting in the hard rock in the form of a flat shore formation, in, *e.g.*, Baumann Fjord and to the north of the Seventeenth of May Hill (Syttende Mai Haugen). The same phenomenon is exhibited in the foreland, which, to the breadth of 1 to 3 miles, and to the height of 650 feet, encircles the plateau, especially in many localities in Eureka Sound. It is also remarkably plain to see on Graham Island.'

5. *Prevalence of the Dome Form in Mountain Uplifts.*—Israel Russell, in a preliminary paper on the geology of the Cascade Mountains in Northern Washington, says:—

'The great block of the earth's crust, 100 to perhaps 150 miles broad, and with a much greater but as yet unknown length, with an elevation of from 7,500 to 8,000 feet, from which the Cascade Mountains as we now know them have been sculptured, was a nearly flat-topped elongated dome.' This is the generalised form of the Cascade region, but in portions of the uplifted area there are secondary elevations 'which are in the nature of secondary domes or protuberances on its surface and flanks.' ('Twentieth Ann. Rept. of the U.S. Geo. Survey, 1898–99,' pp. 98–99. Part II. 'General Geology and Paleontology.')

6. *The Original Form of Sedimentary Deposits.*—In a paper under this title published in the 'Geological Magazine' (January and February 1903), the Rev. J. F. Blake seeks to show that sedimentary deposits are not thickest near to their source of

supply, and that the inferences of geologists in this particular are wrong. The physical reasoning by which he arrives at this opinion is worthy of study.

In this connection and the continental shelf problem I should like to call the reader's attention to pages 290–292 of this work, reprint of 'The Atlantic as a Geological Basin,' Presidential Address to the Liverpool Geological Society, 1884–5.

7. *The Slate Belt of Eastern New York and Western Vermont.*—This report by T. Nelson Dale will well repay study, containing as it does so much practical information relating to slates and slate quarrying, as well as displaying an intimate systematic knowledge of the physics of slaty-cleavage. ('Nineteenth Ann. Rept. of the U.S. Geo. Survey, 1897–98.' Part III. 'Economic Geology.')

8. *Experimental Geology.*—Mr. F. W. Rudler, F.G.S., in a Presidential Address to the Geologists' Association, 1889, gives a short history of the development of experimental geological research which should be read by those interested in the subject and desirous of understanding its aim and scope.

9. *Continental Plains.*—Speaking of the railway systems of South Africa, the special commissioner of the *Engineer* says:[1] 'You can travel for thousands of miles without the sign of a tunnel and almost without cuttings or embankments. The rise, too, is as a rule so gradual from the sea-level to the plateaux of the interior that, although the railway to Johannesburg reaches an altitude of 5,689 feet, the gradients are not often severe. This is all the more unusual when we bear in mind that by far the greater portion of these lines *follows the natural undulations of the ground, being laid upon the surface,* with merely a sufficiency of ballasting to maintain the road as a permanent way.'

Mr. John Foster Fraser, in his interesting and informing book 'The Real Siberia,' gives graphic sketches of the Siberian plains as seen from the Trans-Siberian railway:—

'If you have been on a steamer in a dead calm, and seen nothing but a plain to the edge of the world and heard nothing but the thump-thump of the engines, you will understand exactly how traversing Western Siberia impresses one' (p. 27).

'Once—only once from Moscow to Streitinsk—we ran

[1] February 3, 1903, p. 157.

through a bit of a tunnel not a hundred yards long' (p. 137). (The distance is 4,055 miles.)

10. *Temperature of the Earth.*—Mr. R. L. Jack, in the discussion of a paper by Mr. Whitaker 'On some Well-sections in Suffolk,' read to the Geological Society of London (December 1902), said of the borings in Queensland, Australia: 'In the last seventeen years 202 miles of bores had been sunk to depths varying up to 5,045 feet; 532 successful bores gave an annual outflow of 128,022,767,710 gallons. The temperature of one of the bores was 196° Fahr.'

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